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THE DEATH-WARRANT OF The French Theory of Chemistry,

SIGNED BY

TRUTH, REASON, COMMON SENSE, HONOUR AND SCIENCE.

WITH A THEORY

Fully, clearly, and rationally accounting for all the Phenomena.

ALSO,

A full and accurate Investigation of all the Phenomena of Galvanism;

AND

STRICTURES UPON THE CHEMICAL OPINIONS OF

Messrs. Weigleb, Cruickshanks, Davy, Leslie, Count Rumford, and Dr. Thompson.

LIKEWISE,

REMARKS UPON MR. DALTON'S LATE THEORY,

AND OTHER OBSERVATIONS.

BY ROBERT HARRINGTON, M.D.

MAGNA EST VERITAS ET PRÆVALEBIT.

NOTHING EXTENUATE,
NOR SET DOWN AUGHT IN MALICE.

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October, 1804.

THE ROYAL INSTITUTION OF GREAT BRITAIN

OBSERVATIONS

ON THE

FRENCH CHEMISTRY.

THERE has been for ſome time a very earneſt and violent controverſy amongſt chemiſts, in reſpect to the phenomena which take place from the operation of the ſteam of water paſſing through heated bodies.

Weigleb, a famous German chemiſt, by paſſing the ſteam of water through an earthen tube of ſmall diameter, heated quite red hot, a conſiderable quantity of air was generated, which conſiſted almoſt entirely of azote gas, mixed with a ſmall quantity of carbonic acid, or fixed air: alſo, by the ſteam of water paſſing through manganeſe, the ſame phenomena took place. Theſe phenomena, he ſaid, directly contradicted Lavoiſier's theory, ſhewing it to be falſe. The ſtrenuous advocates of that theory as warmly defended it; ſo that all the continent were at war upon the iſſue, and the conteſt was very warmly carried on, ſome embracing the one opinion and ſome the other. Since this French pneumatic theory has been given to the public, it has given birth to many of theſe controverſies; and as all the combatants have imbibed wrong principles, they have contradicted the obvious and clear facts of each other.

Their experiments clearly prove this; that the ſteam of water, by paſſing through earthen tubes, produces air, principally what they call azote, with a little carbonic acid, and ſometimes partaking a little of the nature of atmoſpheric air. The French theoriſts ſay, that this air came from the atmoſphere paſſing through the earthen tubes being made perambeable by the ſteam of water: but the experiments clearly contradict this opinion; for, if the earthen tube is glazed upon the outſide, nevertheleſs air is generated. But the German chemiſts endeavoured to eſtabliſh the idea, that the air was generated by the water and fire alone, and that any heated bodies, by having water thrown upon them, would generate this air. This the Dutch chemiſts proved was erroneous, as, the ſteam paſſing through glaſs tubes, no air was generated; therefore their hypotheſis was falſe. Alſo, to ſhew clearly that the earth was neceſſary in the proceſs, that the ſteam ſhould act upon it; for, if the tubes were glazed in the inſide with the ſame malleable glaze as the outſide, they found no air was generated; for this reaſon—it cut off the action of the ſteam upon the earth, juſt acting as a glaſs tube. I would wiſh my learned reader would examine the writings of theſe controverſialiſts, and he would find the facts accurate, as I have ſtated them. Here then is the true ſtatement of the controverſy, each diſputing the direct *facts* ſtated by the other.. Now, let us ſee if my theory will not account for the phenomena, upon thoſe undoubted *facts* which were clearly proved by each; alſo by Dr. Prieſtley's experiments, and by Dr. Girtanner's, who ſays, "I muſt firſt confeſs, before I enter upon "this diſcuſſion, that the manner in which the production "of azote gas has been explained, in ſuppoſing the exter-

"nal air to paſs through the tubes, did not appear to "me at all ſatisfactory.—I had myſelf formerly adopted "this explanation; but I very ſoon gave up an opinion ſo "contrary to every thing we know of chemiſtry.—I am "really concerned to ſee this improbable opinion main-"tained by chemiſts of the firſt rank. But the hiſtory "of chemiſtry ſupplies us with many examples of the "ſame nature."*

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* Though Dr. Girtanner is ſo devoted a theoriſt to the French chemiſtry, yet he is forced to acknowledge the truth of theſe experiments; but being determined to explain them upon French principles, he ſuppoſes that azote is a compound body; the abſurdity of which the French chemiſts are aſhamed to adopt, and therefore refute him. But hear, O ye learned! (our alchemiſts were nothing to our *aerial* philoſophers:) he ſays, "There are many other opinions, "long ſince exploded, to which we muſt now have recourſe. That, "for inſtance, of the tranſmutation of metals. What chemiſt, of the "preſent day, would preſume to deny the poſſibility of it. The "change of one metal into another ſhould appear leſs difficult than "the changing of the ſweeteſt body (ſugar) into the soureſt body "(the oxalic acid); than the change of the hardeſt body (the dia-"mond) into the ſofteſt body, the carbonic acid gas; or than the "changing the moſt tranſparent body, the diamond, into coal, the "moſt opaque of all bodies. In the nineteenth century, the tranſ-"mutation of metals will be generally known and practiſed. Every "chemiſt, every artiſt, will make gold. The utenſils for our kitchens "will be made of ſilver, or even of gold, and this will contribute, "more than any thing elſe, to prolong life, which has hitherto been "poiſoned by the oxyde of copper, lead, and iron, which we daily "ſwallow at our meals. There will then be no other wealth than "natural wealth, the production of the ſoil. Artificial wealth, ſuch "as gold, ſilver, or paper, will be extinguiſhed in the hands of thoſe "who have accumulated them. What a revolution in ſociety! But "every enlightened chemiſt will agree with me, that this revolution "is not only probable, but near." — I can only ſay to ſuch theories, *riſum teneatis amici.*

Thoſe earthen clay tubes or veſſels all poſſeſs a quantity of fixed air, or carbonic acid gas*; by being violently heated, or red hot, the fixed air gets a ſaturation of fire, ſo as to neutralize its acid; and when the ſteam of water is thrown upon them, the water penetrates into the tubes, and gives the air its water of compoſition, ſo that it leaves the tubes, and ſhews itſelf as air having different ſaturations of fire, principally as azote, but ſome as high as atmoſpheric air; and other parts none, ſhewing itſelf as pure fixed air. And thoſe earths that have given out all the air that heat can force from them, when ſteam is applied to them, the air that could not be forced out by the dry heat, now, by the action of ſteam, the condenſed airs in the earths will receive their waters of compoſition, and take on the form of air. Moſt all earths contain both fixed air and water, and while the water continues it will give to the air its water of compoſition, but when that is exhauſted they will require ſteam. Dr. Prieſtley, by paſſing ſteam through barytes, produced the greateſt quantity of fixed air upon the ſame principle.

To prove this beyond all controverſy, from their own experiments, which I am moſt anxious to bring forward, as they cannot have the effrontery to contradict them. By paſſing the ſteam through manganeſe, they found the following phenomena:—

Dr. Prieſtley ſays, "With manganeſe, however, the "reſult was different. Having made the ſteam of four

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* Dr. Prieſtley ſays, "Oil of vitriol has been ſuppoſed to enter "into the compoſition of clay. From four ounces of it I got 20 ounce "meaſures of air, in every portion of which one-tenth was fixed air, "and the reſt of the ſtandard of 1.92, 1.72, and at laſt 1.44."

“ ounces of water paſs over 828 grains of this ſubſtance, “ which had been expoſed to a ſtrong heat in an earthen “ retort ſome time before, I got 35 ounce meaſures of “ air, almoſt the whole of which was pure fixed air, “ with a reſiduum a little better than common air. The “ manganeſe had loſt 132 grains, and from being black, “ was become very brown. Again, I tranſmitted the “ ſteam of eight ounces of water over two ounces and a “ half of manganeſe, and got about 100 ounce meaſures “ of pure fixed air, with a reſiduum a little phlogiſti- “ cated. The manganeſe had loſt 112 grains.”

But if this manganeſe is heated, ſo as its fixed air gets ſaturated with fire, without ſteam, it will give out pure air, or what they call oxygen gas; alſo, if is heated, and then ſteam ſent through it, the fixed air will be expelled, as an air with a leſs impregnation of fire, viz. azote, or atmoſpherical air, as the German chemiſts found. Now, in Dr. Prieſtley's experiments, how can our French theoriſts account for its only giving out fixed air by ſteam? But then, when no ſteam paſſes through it, ſo that the water and heat do not expel it, but only heat it, it will then get ſuch a high ſaturation of fire, as to form the pureſt air, or what they call oxygen gas. But when the ſteam paſſes through it, while it is previouſly heated red hot, an impure air, or azote, will be formed.*

Now can *facts ſpeak more forcibly?* How could the manganeſe give out only fixed air by ſteam, and pure air by heat alone, without the water? Could ſteam carry with it the carbone neceſſary to turn the manganeſe pure air, which they ſay it contains, into fixed air?—You wiſe theoriſts! how can you reconcile theſe undoubted facts? For the ſake of juſtice, honour, candour, and ſincerity, acknowledge truth, when it is ſo clearly placed before you!—This is the caſe with many other bodies, even by the application of fire alone, without the aid of water; as the calces of lead, for inſtance. If the grey calx of lead is expoſed to fire, it will part with the air it received from the atmoſphere in its calcination as fixed air, the air having loſt in the carbonation, or combuſtion, its fire which ſaturated it. But if this calx is expoſed to a reverberatory furnace, ſo as to have its fixed air ſaturated with fire, and to become red lead, it will then have its fixed air expelled by heat, as pure air. Every chemiſt knows that limeſtone is formed of calcareous earth, fixed air, and water, ſo that the new chemiſtry calls it the carbonate of lime. If the diluted vitriolic acid is applied to it, it gives out the pureſt fixed air; but if heat alone is applied to it, part of its fixed air is ſeparated from it, as azote and pure air. Dr. Prieſtley ſays, (vol. vi. p. 226)

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* If fixed air is paſſed through heated manganeſe, it will become pure air.

"Nothing in the form of a ſtone yields ſo much air "as limeſtone, and this is by no means all fixed air, as I "believe has generally been ſuppoſed. For a very great "proportion of it is more or leſs phlogiſticated, and the "laſt portions often tolerably pure, ſo that a candle "would nearly burn in it.

"From four ounces of white cryſtals of limeſtone I "got 830 ounce meaſures of air, the firſt portion of "which had only one-fourth of fixed air, but in the "courſe of the experiment it varied, being once three-"fourths, then one-half, and at the laſt one-third. The

" ſtandard of the reſiduum was never better than 1.56, " nor worſe than 1.66.

" From five ounces and a half of limeſtone of an " excellent kind, I got in all 1160 ounce meaſures of " air. Of this one-tenth only was phlogiſticated, and " the reſt fixed, but the laſt portion of all was half phlogiſticated.

" From ſeven ounces of a a tranſparent ſubſtance, " found in a ſtone in the neighbourhood of Oxford, " which is chiefly calcareous, I got 1280 ounce meaſures of air, of which about one-third of the whole " was fixed air. The ſtandard of the reſiduum was at " firſt 1.55, and afterwards 1.44.

" From ſix ounces of a blue ſtone, found in the " neighbourhood of Stratford, I got 1030 ounce meaſures of air, of which, till near the end of the proceſs, " about one-half was fixed air, and the laſt about one-fourth. The ſtandard of the remainder was about 1.6.

" From three ounces of chalk I got 630 ounce meaſures of air, of which at the firſt one-fourth was fixed " air, then almoſt two-thirds, then ſomething more " than one-half, and again a little more than a third. " The ſtandard of the reſiduum was from 1.66 to 1.34.

" The reſiduum of the air from other ſtony ſubſtances, after the fixed air is ſeparated from it, I have " always found to be phlogiſticated, but that from iron " ore is inflammable."

In Dr. Prieſtley's experiment, in expoſing fixed air to heat, part of it was turned to azote.—[See Nicholſon's Journal, in his conteſt with Mr. Cruikſhanks.] Lead, when calcined, turns firſt to the grey calx, and then to maſſicot; and if not calcined with too active a fire, forms red lead; that is, a fire with a reverberatory heat, aided by water, the heat gradually ſaturating the fixed air of the calx; but if too active a fire, it hinders the ſaturation of the fixed air and fire, and litharge is formed, which retains the air as fixed air. I could bring fifty other experiments in chemiſtry, but they are all unneceſſary: thoſe who will not be convinced by theſe, muſt be impenetrable to conviction; and ſhut their eyes, from the *worſt of motives*, againſt every honourable principle. Candid reader, what do ſuch perſons deſerve?

Now let us conſider that ſuppoſed intereſting experiment of Mr. Lavoiſier, which was the great cauſe of his ſyſtem. In adding the nitrous acid to mercury, nitrous air is produced; after that, expoſing the ingredients to the action of fire, oxygen gas is produced, and the mercury revived; and upon mixing the nitrous air and the oxygen gas, the nitrous acid is produced again. But Mr. Fourcroy ſays, in his Elements, "There is, "however, a difficulty attending this opinion. Lavoiſier "did not ſucceed in recompoſing all the acid he made "uſe of, but loſt at leaſt the half, and the quantity of "pure air obtained was much more than neceſſary to "ſaturate the whole quantity of nitrous gas. He con"feſſes that he cannot account for this circumſtance." This is the proceſs which produces the pureſt oxygen gas. Therefore we ſee that their great leading experiment is very deficient in accounting for the phenomena according to their ſyſtem. But let me examine it a little more accurately than it has been hitherto done.

The calx of lead and mercury, alſo manganeſe, give oxygen gas when heated. The two former, when calcined, imbibe the pure air of the atmoſphere; but the mercury is the only calx that gives back very pure air when reduced. Now this is certainly a very ſingular fact, and requires inveſtigation. And even when mercury is calcined into a black powder, by being triturated in the open air, it gives out pure air, and a kind of impure air, but no pure dephlogiſticated air. (See Dr. Prieſtley.) Alſo, as he ſhews that mercury, when mixed with a very little lead or tin, are, by triturating, calcined, he ſays, "The firſt thing that occurred to me "to do with it was to endeavour to expel air from it by "means of heat. Accordingly, I put a quantity of it "into a glaſs phial with a ground ſtopper and tube, "and, with the heat of a candle, I preſently expelled "from it a quantity of air; which being admitted to "lime-water made it very turbid, and was, in a great "meaſure, abſorbed; a proof that the air it had contained was in part fixed air, and the remainder was "not ſo much diminiſhed by nitrous air as common air "would have been; ſo that no pure air came from this "black powder, and conſequently it differed eſſentially "from the precipitate per ſe, which would have yielded "no fixed air, but the pureſt dephlogiſticated air only.

"Expoſing another part of this black powder to a "red ſand heat in a glaſs veſſel, I produced a greater "quantity of fixed air."*

Now, from whence could come the charcoal to form the fixed air; and alſo, more extraordinary, from whence could come the azote, as there appeared little oxygen gas. But when the mercury is calcined by heat, viz. the precipitate per ſe, the air that comes from it, at its reduction, is pure oxygen gas, at leaſt only little of either azote or fixed air.

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* Dr. Gren ſays, (vol. ii. p. 221) "It follows from this that in "the preparation of the mercurial oxyd, the heat muſt not be urged "to the intenſity of red heat." This ſhews, even in this volatile metal, too great a heat will deſtroy that delicate ſaturation of the fixed air being ſaturated with fixed fire.

In theſe inveſtigations I am not bringing forward my own experiments, but thoſe of others—their own acknowledged ones, and therefore they cannot diſpute them. Dr. Prieſtley ſays (vol. ii. p. 48), "My next "inquiry was, by what means it comes to be ſo pure, or, "philoſophically ſpeaking, to be ſo much dephlogiſticated; and ſince the red lead yields the ſame kind of "air with mercurius calcinatus, though mixed with "common air, and is a much cheaper material, I proceeded to examine all the preparations of lead, made "by heat in the open air, to ſee what kind of air they "would yield, beginning with the grey calx, and ending with litharge.

"The red lead which I uſed for this purpoſe, "yielded a conſiderable quantity of dephlogiſticated "air, and very little fixed air; but to what circumſtance in the preparation of this lead, or in the "keeping of it, this difference is owing, I cannot "tell. I have frequently found a very remarkable difference between different ſpecimens of red "lead in this reſpect, as well as in the purity of the air "which they contain. This difference, however, may "ariſe in a great meaſure from the care that is taken to "extract the fixed air from it. In this experiment two "meaſures of nitrous air being put to one meaſure of "this air, reduced it to one third of what it was at firſt, "and nearly three times its bulk of nitrous air made "very little addition to its original dimenſions; ſo that

“ the air was exceedingly pure, and better than any that “ I had procured before.

“ The preparation called maſſicot (which is ſaid to “ be a ſtate between the grey calx and the red lead) alſo “ yielded a conſiderable quantity of air, of which about “ one half was fixed air, and the remainder was ſuch, “ that when an equal quantity of nitrous air was put to “ it, it was ſomething leſs than at firſt; ſo that this air “ was about twice as pure as common air.

“ I thought it ſomething remarkable, that in the “ preparations of lead by heat, thoſe before and after “ theſe two, viz. the red lead and maſſicot, yielded only “ fixed air. I would alſo obſerve, by the way, that a “ very ſmall quantity of air was extracted from lead ore “ by the burning lens. The bulk of it was eaſily ab- “ ſorbed by water. The remainder was not affected by “ nitrous air, and it extinguiſhed a candle.

“ I got a very little air by the ſame proceſs from “ the grey calx of lead, of preciſely the ſame quality “ with the former. That part of it which was not af- “ fected by nitrous air extinguiſhed a candle, ſo that “ both of them may be ſaid to have yielded fixed air, “ only with a larger portion than uſual, of that part “ of it which does not unite with water.

“ Litharge (which is a ſtate that ſucceeds the red “ lead) yielded air pretty readily; but this alſo was fixed “ air. That which was not abſorbed by water, was not “ affected by nitrous air.”

Now, as Dr. Prieſtley found ſo great a difference in the produce of air from the different calces, and even from the ſame calces, let us ſee if we cannot aſcertain the true cauſe. According to Mr. Lavoiſier’s ſyſtem, they ſhould have only yielded oxygen gas, and neither fixed air nor azote. In placing lead upon the fire in the pureſt veſſel, it ſoon begins to calcine into a grey powder, where it could not poſſibly receive any charcoal*; nay, that in this proceſs it can receive no charcoal is fully proved; that in making minium, or red lead, it requires a long action of the fire, being firſt turned into maſſicot; and then the action of the fire is further required to be long applied to it, nay even to be reverberated upon it, where it might be *ſuppoſed* to have received charcoal. Then the clear and evident deduction, from conſidering the proceſs, is, that the minium is expoſed to a longer action of the fire. — Dr. Gren ſays, "The red oxyd, "commonly called red lead (minium), ſeems to be the "moſt perfect oxyd of this metal. It is prepared from "maſſicot recently made, and while yet hot; which, in "order to promote its cooling, is previouſly moiſtened, "and then elutriated with water. Upon this it is "roaſted ſlowly, and for a conſiderable time, generally "48 hours. The metal ſhould not be heated to redneſs, "as in that temperature it would be converted into "litharge. The colour of red lead is a bright red bor-"dering on yellow." Now here it appears, that maſ-"ſicot, which gives out only fixed air and azote, is, by the action of the fire, formed into minium. This, they ſay, is from receiving a more complete calcination, imbibing more oxygen gas. But that the next and laſt ſtep of further calcination is called litharge, and which, inſtead of receiving more oxygen gas, poſſeſſes none of it, but only fixed air and azote, directly the reverſe to what it ought to be, according to their hypotheſis.†

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* But this idea is directly refuted by Dr. Prieſtley's experiments, juſt quoted, where the calcination of the mercury and tin took place without fire, by trituration in a glaſs veſſel.

Now let us try it by mine. Pure air (or oxygen gas) of the atmoſphere is formed of fixed air, water, and fixed fire, in a ſtate of neutralization; the fire neutralizing and ſaturating the acid, and the water forming the aerial compoſition, the ſame as it does in the neutral ſalts forming their cryſtallization: then when lead, which is formed of fixed fire united to the earth of lead, is heated to that degree ſo as to produce ignition, the air of the atmoſphere as it burns (as the fixed fire of both the air and metal is actually ſet looſe as free fire, and the earth of the lead has loſt its fire)* it attracts the fixed air and water of the air. But this fixed air is capable of becoming pure again, by being impregnated with fire. And this is done by expoſing the calx to a ſlow and long continued heat, as in the forming of minium or the percipitate per ſe. We need not be ſurpriſed at this, ſince I have formerly *proved* that nature forms this fixed air into pure air again, by various proceſſes, in the upper regions of the atmoſphere; alſo in rivers, and by expoſing water to the ſun. All this is done by the action of the ſun's rays.—(See my former publications.)*

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† Why, when the heat is puſhed further in forming the maſſicot into litharge, is owing to the heat being too ſtrong, as to hinder that ſlow and regular ſaturation of the fixed air, it being a heat tending to combuſtion.

* Heat ſeems the great former of bodies, or conſolidator: it forms the earth of metals into that ſtrong hard metallic ſubſtance; alſo the vitrifying earths into glaſs; and, more particularly, it forms the precious ſtones, and which gives them, as I have proved, that combuſtibility, and to reflect the rays of light with that refulgent ſplendour; and which ſtones are the hardeſt bodies upon earth — they even cut glaſs.

Chemiſts have found a great deal of difference in the quality of the air they get from minium. This is entirely owing to its preparation, by the difference of its calcination: if well and ſlowly roaſted, it will give very pure air, but otherwiſe it will partake more of fixed air and azote. But mercury can only be calcined by a ſlow and regular calcination, owing to its volatility, therefore there is not ſo great a difference in it; but ſtill there is often both fixed air and azote in it. Nay, in all the other calcinations, as iron, &c. I have proved (ſee my Letter in the Gentleman's Magazine for June, 1801,) that they only poſſeſs fixed air and water, producing, with charcoal, Mr. Cruikſhanks' gaſeous oxyd of carbon. But undoubtedly, if reaſon and common ſenſe are conſulted in theſe diſquiſitions, all calces ought to give only pure oxygen gas upon their reduction. To argue otherwiſe is abſurd in the extreme; but I am taught to be ſurpriſed at nothing, even if they were to endeavour to prove by their experiments, that the moon is a large Cheſhire cheeſe; and I am certain they would get the wretched Reviewers to defend it. Now, reader, whoever thou art, whether an expert chemiſt or not, thou muſt ſurely be convinced that, if they will not allow of theſe obvious facts, they ought to be forced at leaſt to refute them. But know, reader, that they expect thy reaſon and common ſenſe muſt be forced to believe *that black is white*, and not queſtion their authority. Thou muſt be gulled—ſwallow all, all, all, even an elephant. O this wiſe age!

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* Many abſurdities have been adopted by our aerial chemiſts, and this one is not the leaſt: that oygen gas makes bodies become red, as minium, mercurius, precipitate per ſe, the red globules of the blood, &c. &c. But every fact directly contradicts it. Sulphur and phoſphorus, by imbibing oxygen gas, become from a bright yellow of a dark, clear, limpid colour, and none of the metals, when calcined by acids, become red. In ſhort, all bodies, either animal, vegetable, or mineral, by burning become black. But many bodies become red by imbibing fire, or at leaſt a bright colour, reflecting the light; as the fixed fire of ſulphur and phoſphorus; all metallic calces, when reduced; mercury, particularly when united to ſulphur; the red globules of the blood: in ſhort, I might mention a thouſand inſtances; but I ſhall conclude with quoting Dr. Prieſtley upon the nitrous acid. He (in vol. vi. p. 2.) proves beyond doubt, that it is either phlogiſton or heat that gives the red colour to this acid, ſo that he ſays, "The "facts that I ſhall relate prove that it is either phlogiſton or mere "heat that gives colour to this acid:" ſo he goes on to prove it, but I refer my reader to the volume. But let me inveſtigate more particularly vitriol of mercury *(ſulfate acide de mercure)*. Sulphur, they ſay, poſſeſſes no oxygen, but the vitriolic acid an immenſe deal. Now, when ſulphur is united to mercury, they form the moſt high rich colour, virmillion; but when the vitriolic acid is united to mercury, they form only a white ſalt, vitriol of mercury.

And the one they ſay poſſeſſes an immenſe deal of oxygen, and the other none. Alkaline ſalts, which are fixed fire, likewiſe give a rich red colour to the calx of metals; for if the cauſtic alkali is added to the preparation of ſulphur and mercury, Kerchoff found that they would form the richeſt colour in nature, viz. the carmine tint.

But, as it might be ſuppoſed that, in forming minium, the maſſicot, by being roaſted, received a greater quantity of pure air, which the burning proceſs would reduce to fixed air, yet (if the proceſs is conducted in cloſe veſſels), nevertheleſs, I found that the maſſicot would be formed into minium; therefore the *moſt bigoted ſceptic* has not the leaſt plea to fix on.

Dr. Prieſtley ſays, " As I was heating a quantity of " minium in an iron ladle, I was very much ſtruck with " the reſemblance of its colour, and of the change of its " colour, to that of blood. The colour of good minium is, " as nearly as poſſible, that of florid, or what I call dephlogiſticated blood. It is the colour they both acquire " from expoſure to the air. When the minium was in " the ladle expoſed to the fire, the ſurface continued of " this colour, but all the lower part of the maſs was of a " deep red, or black, the colour of dark-coloured, or " phlogiſticated blood. But, like blood (only, in this " caſe, the proceſs was much quicker) the moment that " any part of it was turned up to the open air, it reſumed " its florid light colour; and when it was cold, it could " not have been perceived that any thing had been done " to it. However, when I expoſed a quantity of minium that had been treated in this manner to a red " heat, in a glaſs veſſel, though it yielded about the ſame " quantity of dephlogiſticated air that I imagine it " would have done before, it yielded much leſs fixed " air.

" Imagining that this dark colour might be the conſequence of the minium receiving phlogiſton from the " iron, I expoſed a quantity of it to the ſame degree of heat " in a glaſs tube, but found the ſame change of colour. " In this, therefore, it reſembles the change of colour in " ſpirits of nitre, without the help of any additional " phlogiſton, unleſs any may be ſuppoſed to paſs through " the glaſs.*

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* Now, by this proceſs of being longer expoſed to heat, even in cloſe veſſels, it appears it yields leſs fixed air; and as the fixed air

"The tube was ſeveral feet long, and was quite filled with the minium; and preſently after it was "expoſed to the heat of the fire, the colour began to "change, growing darker and darker continually, "exactly as it had done in the iron ladle. But when it "was cold, it re-aſſumed its florid light colour. That "it ſhould do this without the aid of the external air "rather ſurpriſed me; and yet that no air, except what "was contained in the interſtices of the minium itſelf, "had acceſs to it, was evident from the lower part of the "glaſs being ready to burſt with the expanſion of the "air, when it was in a melting heat.

"It was obſervable, that from the black colour, the "minium paſſed, without any ſenſible interval, into "yellow, in which ſtate it contains little or no air of any "kind; ſo that the florid colour is an indication of its "containing pure air, whatever be the connection between theſe circumſtances. It muſt be obſerved, however, that minium deprived of its red colour by ſpirit "of ſalt does not loſe its property of yielding dephlogiſticated air."

If my reader will duly attend to theſe experiments of Dr. Prieſtley, I think he will be fully ſenſible of the juſtneſs of my theory, that, why the calces or oxydes of lead produce different kinds of air, is owing to the fixed air of the calces receiving a ſaturation of fire, ſo as to form it into oxygen gas: for the minium became

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could not eſcape, it muſt either have gone to the formation of oxygen gas, or been annihilated.

Mr. Muſhet has, from his being tied down to the French theory, ſuppoſed, in turning iron into ſteel without charcoal, the abſurd opinion, of its being from the charcoal paſſing through the crucible.

dark coloured by the fire in cloſe veſſels, and after that gave out leſs fixed air. Alſo, that the pure air of the atmoſphere had nothing to do in the proceſs. And alſo, that the idea of the blood becoming florid from the pure air being imbibed by it, I have, I hope, *ſatisfactorily proved* in my former works: it is from imbibing the fixed fire of the pure air, from its ſaturation with its fixed air, and alſo ſetting the water of its compoſition looſe, diſcovered by its moiſture. For, let me aſk my reader this plain and obvious queſtion: can aerial bodies paſs through the veſſels or coats of the blood? But, to bring it home to his obſervation, can air paſs through bladders, which bladders, when filled with blood, will decompound the air, as both Dr. Prieſtley and I found.

But let us now conſider the other body, which yields oxygen gas (viz. mangaueſe), and ſee the phenomena attending it. Manganeſe, according to Dr. Prieſtley (as he is certainly the firſt experimenter amongſt them, I have therefore principally quoted him), yields different kinds of air. He ſays, (vol. iv. p. 203, ſpeaking of manganeſe), "Of this I procured a quantity finely "pounded; and from an ounce of it I got, in a red ſand "head, forty ounce meaſures of air, part of which, in every "portion, was fixed air, and at firſt almoſt wholly ſo; "but four-fifths of the laſt was the pure dephlogiſticated "air. Even the firſt that came over, which was the "common air, in the veſſel was not in the leaſt dephlo-"giſticated. The manganeſe had loſt 1½ dwts. of its "weight, and was not to be diſtinguiſhed in colour "(which was black) from what it had been before. A "conſiderable quantity of water came over during this "proceſs." This was by the action of fire, which clearly changes part of the fixed air into azote and oxygen gas: for, if the air is expelled by ſteam, the air being not expelled by fire, it therefore has its fixed air leſs changed.

As the Doctor ſays (ſee page 4). Now, need I comment upon theſe plain and obvious facts. But, to continue, the Doctor ſays (vol. iv. p. 206—all my quotations are from the Doctor's firſt editions), " I next made trial of ſome " lapis calaminaris, firſt pounding it very finely, then " putting an ounce of it into one of the ſmall long " necked retorts already mentioned, and with a red hot " ſand heat I got from it 306 ounce meaſures of air; " and making allowance for the loſs of air in changing " the veſſels in which I received it, &c. I believe I may " ſtate the whole produce at 316 ounce meaſures, the " whole of which was fixed air, except four ounce " meaſures; and, what I did not at all expect, the re- " ſiduum, after the fixed air had been extracted from it " by water, appeared to be nearly as good as common " air. For one meaſure of it and one of nitrous air " occupied the ſpace of 1¼ meaſures. Had the proper " reſiduum of fixed air been well extracted, the remain- " der would probably have been dephlogiſticated air. " What remained of the lapis calaminars, weighed " 13 dwt. 6 gr. and had a lighter colour than before. " This produce of air I took at different times, but the " reſiduum of the laſt portion was but little better than " that of the firſt.

" From this experiment it appears that lapis calami- " naris, as well as manganeſe, reſembles, in ſome degree, " red lead, which of itſelf, by means of heat only, yields " both fixed air and dephlogiſticated air, only a much " ſmaller proportion of the latter.

" In order to obſerve whether the addition of ſpirit

" of nitre would make any change in the produce of " air, I moiſtened another ounce of it with that acid; " and in a glaſs veſſel, and with a red heat as before, I " got from it 244 ounce meaſures of air, the bulk of " which was fixed air, but twelve ounce meaſures were " not abſorbed by water, and appeared to be dephlogiſti- " cated air.

" It appears from this experiment, that the ſpirit " of nitre, beſides contributing to the production of " dephlogiſticated air, as uſual, contributed alſo to the " production of a large quantity of fixed air, which " favours the hypotheſis of fixed air being a modifica- " tion of the nitrous acid. It will be found, however, " in the courſe of this work, that it is not this acid " only, but the vitriolic acid alſo, that contributes to " the formation of fixed air, as well as of dephlogiſti- " cated air. *N. B.* The ſpirit of nitre made no effer- " veſcence, and produced no heat in mixing with the " lapis calaminaris. It alſo made no change of colour " in it, and a very little of the acid was ſufficient to " make it ſenſibly moiſt; in all which reſpects it differs " very remarkably both from red lead, and from man- " ganeſe."

Here then is lapis calaminaris yielding ſome common air as well as fixed air. But he alſo names another fact, which is clearly aſcertained by numerous other experiments of the Doctor's: that from the nitrous and vitriolic acids fixed air is produced. Upon conſidering the Doctor's volumes, this fact is clearly aſcertained, which we might ſuppoſe *a priori*, as the aerial acid, or fixed air, is only an acid in an aerial ſtate. And that the different acids are formed from one another, is proved from fifty experiments, the ſmell of one acid taking place when the other mineral acids are uſed in the proceſs.—(See Mr. Hoyle's experiments, in the Mancheſter Memoirs, upon the oxmuriatic acid; alſo upon adding the vitriolic acid to the oxmuriatic alkali; and in many other experiments.) Alſo, that the mineral acids are formed into each other, I could bring many proofs*.

But Dr. Prieſtley examined the air from all minerals, both volcanic, baſaltic, and different ores, &c. in ſhort, all mineral bodies, and in their examination he ſays, (vol. vi. p. 230) " Of the air extracted from mangameſe in glaſs " retorts, I gave an account in a former volume. I " now found that ſeven ounces of it, with a ſtronger " heat, in an earthen retort, yielded 110 ounce meaſures " of air, of which at the firſt, a ſmall portion was fixed

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* That the acids may be formed into one another, is diſcovered clearly beyond a doubt. Alſo, that alkalies are formed of earths and fixed fire, is clearly diſcovered in the experiments of the French chemiſts. I have alſo ſeen a Letter from that very theoritical Dr. Mitchell, ſaying, he had detected an earth in the aſhes of burning wood, partaking of half an alkaleſcent ſtate, if I may uſe the expreſſion.

I ſay in my Chemical Eſſays (p. 109), ' In forming the calcareous ' earths into lime, in attending to the proceſs accurately, we may ' obſerve different phenomena; at firſt the fixed air and water are ' expelled, and the earth ſaturated with looſe fire. But if you puſh ' the experiment further, the fire will, inſtead of taking this looſe ſa' turation, become more fixed, penetrating the earth, ſo as to become ' alkaleſcent; and in this ſtate the lime becomes light and ſpungy, hav' ing loſt part of its earth, and will not do for mortar, the workmen re' jecting it, as being uſeleſs; for it will not ſplit and fall into powder ' with water. I have, by carrying on the proceſs for a long time, ' made it almoſt an alkaline ſalt, turning it perfectly mild in reſpect ' to its cauſticity, and having the properties of alkalies in moſt of its ' qualities.'

"air, and of the reſt the ſtandard was in different proportions of it 1.54, 1.25, and 1.05; ſo that the bulk of "it was not perhaps better than common air. The "ſubſtance was concreted into a hard maſs, but was "looſe from the retort." Now, clearly, by a ſtronger heat, the fixed air would get more neutralized with fire, producing principally azote; but, wonderful! azote, they ſay, has nothing to do with mangaueſe, according to their ſyſtem. But Dr. Gren ſays (vol. ii. p. 408), "Carbonic acid likewiſe unites with manganeſe, when "but imperfectly oxyded; and in that ſtate this metal is "diſſolved by water, impregnated with carbonic acid.— "If into a colourleſs ſolution of manganeſe in acids, a "fluid alkaline carbonat be dropped, a white precipitate, being carbonat of manganeſe, falls down. This "is again diſſolved by all acids with ebullition, and "forms clear ſolutions without any colour. But it is "rendered black by calcination." Now here it appears that the manganeſe becomes black from receiving fixed air ſaturated with fire by calcination, and that oxygen has nothing to do in the proceſs, as it will equally anſwer by calcining in cloſe veſſels. But I ſhall ſhew it in another proceſs. Mr. Kirwan (in his Eſſay upon Phlogiſton, p. 52) ſays, "It is impoſſible to deny all credit to "thoſe who aſſerted that lime-water was precipitated by "taking the electric ſpark in common air, though it did "not ſucceed with Mr. Cavendiſh, either from his uſing "an inſtrument of different power from that uſed by "others, or air phlogiſticated by a different proceſs."— Now, permit me to aſk Mr. Kirwan, if he can poſſibly ſuppoſe the electrical ſpark to be charcoal? Yet, according to his theory, it muſt be charcoal, by turning pure air into fixed air. Reader, ſhall I ſay any more?

If thy reaſon is not already convinced, it is nor worth convincing. But I muſt here mention how hard it is to labour at ſuch obvious and clear facts, to ſatisfy the obſtinacy of my readers — what a Herculian labour I have had, when my time ought to be employed in the inveſtigation of nature, not in hammering into the world, that white is not black: but ſuch has hitherto been my fate. What this age will have to anſwer for, being the dupes of this deſpicable combination!

But in terra ponderoſa aerata, Dr. Prieſtley found that he could get no air from it by fire; but by ſteam he got the greateſt quantity of fixed air. It being a heavy earth, implies it contains no water; therefore it wants the water to its aerial compoſition, or why otherwiſe ſhould we not expel its fixed air by heat, as we can do it from all other bodies? No, it wants its water of compoſition, which it gets by ſteam.*

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* Mr. Rupp argues againſt this idea, and ſays that Dr. Prieſtley, in his experiments, did not free the earth from all the muriatic acid. Allow him that he did not; but will he argue that he did not expel by far the greateſt quantity of it? Will he ſay that the ſame quantity remained in it after as before the proceſs; and if a very ſmall part of the acid remained, it would make the earth retain part of the water; therefore it is as broad as long. But why do the acids require to be diluted with water, in order to diſpel the fixed air of bodies, if it was not clearly from giving the fixed air its water of compoſition. If Mr. Rupp will apply my theory to his remark on Dr. Prieſtley's analyſis, he will find his able objections totally removed.

I once named Mr. Rupp before in my writings. I thought he had haſtily committed himſelf, in ſaying that lime water became muddy in one of Mr. Henry, junior's, experiments. But his anſwer to Dr. Prieſtley's theory I think a very candid and able one. As to his experiments on the compoſition of neutral ſalts, they are ſo obſcure a

But it is well known that the calcareous earth contains both fixed air and water; for, if lime is expoſed to fixed air or water ſeparately, it will attract neither; the fire it is ſaturated with hinders attraction of either of them ſeparately; but by their joint influence they expel the fire, and form the lime into a limeſtone again.[*] Now there is not a better atteſted fact, which we owe to Dr. Black, than that the calcareous earth contains pure fixed air; for, by expelling it by the watery acids, the pureſt fixed air is expelled, and no other. If we diſpute this plain fact, there is no agreeing. Then, when this fixed air is expelled by fire, what kind of air does this fixed air become.

ſubject, that nothing can be relied on with reſpect to them. But nothing can be more obvious than theſe experiments. See the French chemiſts, how they differ from Kirwan's experiments upon the compoſition of neutral ſalts: but indeed no chemiſts agree concerning them, they are all at variance. Mr. Hope is ſaid to have got fixed air from barytes by fire alone. I deny the fact. But his authority is very poor indeed. Even if we could obtain *a very ſmall quantity*, it would avail nothing, as it might have received a little moiſture from the air.

[*] Such is the great credulity the public are under to this deſpicable combination, that they will diſpute that black is black: but if we are to believe that one and one make two, we muſt know that calcareous earths conſiſt of fixed air and earth, therefore expelling the fixed air by acids, or by fire. What a great difference it makes: the former expelling the pure aerial acid, and the latter as part of it, ſaturated with fire, as azote, and even as atmoſpheric air. But if the public are unwilling to open their eyes, let them take calcined lime, and expoſe it to fixed air and water, and they will form the pure calcareous earth again, or what they call aeriated limeſtone. If I am not to be allowed the common facts of our forefathers, there is no arguing. But I am ſurpriſed at nothing. For the ſake of ſcience, honour, juſtice, and truth, throw off the influence of this mean combination.

* Need I comment upon what appears ſo obvious and plain to every capacity? Can there be any ſo puerile as to miſtake it, after this full diſcuſſion? I ſhould ſuppoſe not; it would be only to inſult my reader's common ſenſe. I ſay none but this baſe combination, and I think *even they* cannot be ſo bare-faced as to do it. But I ſhould be ſurpriſed at nothing they do; reaſon, common ſenſe, truth, juſtice, and humanity, are nothing to them, and their mean, deſpicable agents, the Reviewers. What will they ſay to all this, when they review me? *Nothing*, but tell their *poor deluded readers*, that it is all abſurd nonſenſe; that I am no inhabitant of their wonderful French iſland of Laputa. *Will the public be gulled again?* For my part I ſhall not be ſurpriſed at any thing.

The experiments juſt quoted of Dr. Prieſtley (p. 15 and 16) prove my theory, and directly contradict theirs. They ſhew that heat deepens the red colour of minium from the florid light colour, and that when turned up to the air to cool, it became lighter again; that all theſe changes take place in cloſe veſſels, where no air is expelled from it, and no air is received by it; and that even by the heat being ſtrong it became yellow, or maſſicot, which the French theory expreſsly ſays can only take place by parting with its oxygen gas. But the maſſicot, it appears, can be formed into minium in cloſe veſſels, and that minium again into maſſicot, without receiving air and im-

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* The old chemiſts have been greatly ridiculed in their opinions and attempts to turn metals into gold; but I will aſſert, that their opinions were not half ſo abſurd as the one of ſuppoſing diamonds are charcoal. Diamonds have a ſtronger concentration of fire than charcoal, therefore require a greater proportion of pure air to burn them, and in conſequence leave a greater proportion of fixed air, and not, Mr. Conjurers, that they are both formed of carbone.

*parting air ; the maſſicot receiving fire, which its fixed air and water fixes, forming it into pure air, forming minium ; and when that minium is expoſed to a ſtrong heat its fixed fire is diſſipated, from its union with the fixed air, and is again ſet free.** And manganeſe, which they acknowledge contains an immenſe deal of iron, becomes black inſtead of red, from being what they call oxygenized. Here is a direct fact againſt their puerile theory. In ſhort, nothing can ſhew greater abſurdity and puerility than their opinions.

Fire, when fixed in bodies, often changes the colour to red, as, in reſpiration, the fixed fire of the air changes the colour of the blood, as I have clearly ſhewn in my former works. Either fixed or free fire has the ſame effect upon the nitrous acid, turning it red. The ſun's rays have juſt the ſame effect upon fruit; the gooſeberry or currrant for inſtance. Nay it clearly appears that manganeſe, which they allow contains a great proportion of iron, when it contains pure air, is black, as it is only the black manganeſe that gives out pure air, the other only fixed air.

* Our modern aerial chemiſts, in accounting for this change in the red globules, ſay it is owing to the iron the red globules of the blood contain, merely arguing from analogy, as they ſay it has this effect upon lead. But, by the beſt chemiſts who have analyzed the red globules, they contain no iron; but, according to them, iron becomes black by oxygen gas, as in finery cinder, which they ſay has got one third of its weight of oxygen gas. And even as to ruſt of iron, it is not a ſcarlet red.

But how beautifully do the experiments in the elaboratory and thoſe in nature agree. In lead, its firſt calcination is a grey colour; but by receiving fire becomes maſſicot and yellow; by receiving more fire becomes minium, or a bright yellow red; and by more fire, becomes of a dark red; but when it cools becomes of a yellow red again. — (See Dr. Prieſtley's experiments, juſt quoted (p. 16 and 17.) In nature let us ſee the action of fire upon vegetation. Fruit, when it begins to ripen, turns firſt gradually yellow, and then gradually red, as it gets ſaturated with the ſun's rays; the ſame in many other proceſſes in nature, which might be named. But their theory is not only *in direct oppoſition* to the different chemical proceſſes, but alſo to all the phenomena in nature.

Mr. Chevenix has given a long paper in the Philoſophical Tranſactions upon the oxmuriatic acid, as they call it. According to him, one of the ſtrong mineral acids, the muriatic acid, contains no oxygen, though phoſphorus and ſulphur, ſuch ſtrong phlogiſtic bodies, by this conjurer oxygen, form ſuch ſtrong acids with it they ſay. But, ſtill more wonderful and inconſiſtent, the hyper-oxmuriatic acid, from receiving an immenſe deal of oxygen, loſes all its acidity. This wonderful *hocus pocus conjurer* then, in moſt inſtances, turns phlogiſtic bodies into the ſtrongeſt acids in nature, viz. the ſulphuric, &c.: but another equally ſtrong mineral acid is by this ſame body bereft of all its acidity. Dean Swift, I am certain, had no ſuch legerdemain work upon his flying iſland: no, this *hyper* honour was left to *our hyper aerial chemiſts.** The proceſs is ſimply this: the marine acid, by being diſtilled with manganeſe, paſſes over into the receiver, with a part of the metallic manganeſe; and this ſtrong acid then having its acid neutralized in part with the manganeſe and fire, which, together, gives the acid that active penetrability, as when it meets combuſtible bodies, that is, bodies formed of fixed fire, it will penetrate theſe bodies, and ſet their fixed fire looſe as free fire, producing combuſtion. This I think was clearly the caſe beyond all cavil, in what he is obliged to ſay in the Philoſophical Tranſactions.

That this is clearly the caſe, for, if we are to allow the leaſt conſiſtency in their theory, that oxygen makes bodies more acid, as they allow that the acidity of the nitrous and the vitriolic acids depend upon the quantity of oxygen they contain; they naming them by this proportion of oxygen as nitric and vitriolic acids. But ſtill more inconſiſtent their theory is; for it is allowed that the ſtrength of the acid's attraction for earths and alkalies depend upon their acidity, or, according to them, to their oxygency. This is one of the firſt and ſtrongeſt facts in chemiſtry. But, wonderful! the common marine acid has a very ſtrong attraction for alkalies and earths, expelling the weaker acids; but, by receiving more of the acidifying principle, it loſes not only its acidity, but alſo loſes its attraction for alkalies and earths. Does not this forcibly tell thoſe that are open to common ſenſe, that the oxmuriatic acid is united to the earth of the manganeſe or lead, and the fire, as to be partly neutralized, ſo that it cannot ſhew much acidity or attraction for alkalies or earths; and when it is powerfully aſſiſted by heat, it drops part of its earth, and attacks the alkali, ſo as to form the hyper-oxmuriatic acid.

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* See my obſervations on Volta's Pile (p. 38), where Mr. Cruikſhanks produced it, and I challenge him to ſhew that it could poſſibly receive any oxygen, but really was a deoxygenating proceſs.

Mr. Hoyle, in the Mancheſter Memoirs, who was the firſt, I believe, who ſhewed the proceſs of making this hyper-oxmuriatic acid, found (ſee Mancheſter Memoirs, vol. v. p. 224)† that when the oxmuriatic acid united to the alkali, it parted with a part of the manganeſe, and united itſelf to the alkali, as the alkali is a body with a far ſtronger ſaturation of fixed fire, as I have proved. It is from this ſame cauſe that nitre produces ſo much more oxygen gas than when the nitrous acid is united to earths.—(See my former works) Alſo, in Nicholſon's Journal, Mr. Murray has written a long diſſertation upon paſſing the oxmuriatic acid through ſulphur; the acid united with the ſulphur, and formed a peculiar body, formed of ſulphur, the muriatic acid, and a ſmall proportion of manganeſe; but, according to their theory, it ought to have formed the ſulphur into the vitriolic acid. But he ſays, "Sulphurated muriatic "acid then contains

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† He ſays, "Before any of the ſalt appeared to be formed in the "alkaline ſolution, I have conſtantly obſerved a quantity of earthy mat-"ter to be precipitated."

Alſo, (p. 230) "Finding a great quantity of gas to be diſengaged "from this ſalt by the ſulphuric acid, which had a very ſtrong ſmell of "nitrous gas, I put forty grains of the ſalt into a glaſs retort, and poured "upon it nearly an equal weight of ſulphuric acid, diluted with water.

"44.00	oxyde of ſulphur
35.75	muriatic acid
20.25	loſs
10.000"	

And this oxyde, he ſays, "though the reſult of a "ſimilar experiment, was nearly the ſame; yet the pro-"portion of oxygen is certainly too ſmall, to authoriſe

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"With the heat of a lamp the gas began to come over very rapidly, and "was received in a glaſs jar placed in a baſon of water. A conſiderable "portion of it appeared to be abſorbed by the water, which acquired a "yellowiſh colour. This colour diſappeared on ſtanding a few days, and "a brown matter was depoſited, which being carefully collected and "dried, weighed one grain, and appeared to be manganeſe; for, a little "of it being put into the muriatic acid, ſo far oxygenated it, that it would "deſtroy the blue colour of a diluted ſolution of indigo in the ſulphuric "acid."

And alſo it appears that the nitrous gas is produced when there was no nitrous acid, but only the muriatic and ſulphuric acids.

" us, in the preſent ſtate of chemical analyſis, to con-" clude that the ſuppoſed oxyde really contains $2\frac{1}{2}$ per " cent. of oxygen: or ſo ſmall a deviation from the " compoſition of ſulphuric acid, by acidifying common " ſulphur, as $2\frac{1}{2}$ per cent. may, very probably, be owing " to an error of analyſis."

Then what becomes of their conjurer oxygen in the proceſs: he is obliged to ſay (Nicholſon's Journal for 1803, p. 108), " The ſulphuret of muriatic acid claims the " peculiar attention of chemiſts, not only on account of " its compoſition, which our previous knowledge would " have induced us to conſider as impoſſible, but on ac-" count of the many remarkable properties which it " diſplays." But even let us advert to Mr. Chevenix's own experiments and obſervations, and I think, reader, if thou poſſeſs the leaſt candour and common ſenſe, thou muſt think them very deciſive againſt the French theory. He ſays (Philoſophical Magazine for April, 1802, p. 218), " With regard to efficacy and prompt-" neſs, tin or zinc is preferable to iron; but, with any " of the three metals, a phænomenon occurs, for which " I have not been able to account in any manner the " leaſt ſatisfactory. As the effects are more ſtriking " with zinc than with the other metals, I ſhall enter " into particulars reſpecting the uſe of that metal only.

" When a bit of zinc (or tin, or iron), is immerſed " in a ſolution of muriate of copper, a precipitation " begins, and all the copper is finally ſeparated in a few " hours*; but, if a little muriatic acid is added to this " ſolution, and the zinc then immerſed, a violent effer-

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* " If any quantity of neutral ſalt is preſent in the ſolution, the preci-pitation is much retarded, and is ſeldom complete."

" veſcence takes place; hydrogen gas is diſengaged, " and, in leſs than two minutes, the copper is ſo completely precipitated in the metallic ſtate, that neither " ammonia, nor even ſulphurated hydrogen, can diſcover any veſtige of its preſence.

" It would appear from this, that hydrogen is, in " this caſe, the real reductive and precipitant of copper. " Yet, how can we reconcile the evident contradiction, " that, in one caſe, zinc with muriatic acid will decompoſe water, that is, that zinc and muriatic acid will " attract oxygen more ſtrongly than hydrogen can, " yet that, in the other caſe, hydrogen, whoſe affinity for " oxygen is weaker than that of zinc and muriatic acid, " will be more ſpeedy and powerful than zinc and muriatic " acid, in attracting that oxygen from copper? Again, " how is it poſſible that zinc and muriatic acid will, in " preference to taking oxygen immediately from the " oxyde of copper, decompoſe water, the hydrogen of " which will unite with the oxygen of the oxyde of copper, again to become water, which it originally was? " All this appears to me, I confeſs, as contradictory as " to ſay, one is leſs than two, two are leſs than three, " yet three are much leſs than one. This opinion, that " hydrogen is really the reductive, is the more extraordinary, as it is not founded on the ſingle experiment " above mentioned.

" If a bit of zinc (or tin, or iron), is thrown into a " ſolution of oxyde of arſenic in water, no change of any " kind will be effected, even after a conſiderable time: " but the inſtant that muriatic acid is added, efferveſcence and precipitation commence; a few minutes " ſuffice to obtain all the arſenic in its metallic ſtate. It " may be objected here, that muriatic acid (as muriatic

" acid) produces ſome hitherto unknown modification " in the order of eſtabliſhed affinities. This objection " is not plauſible; and I can deduce ſo clear an anſwer " to it, as to ſhew that it is of very little moment. If " aquous arſenic acid is uſed inſtead of the above ſolution, " the ſame phænomena of effervefcence and precipi- " tation enſue as when muriatic acid had been uſed in " the former caſe; and that preciſe quantity of metallic " arſenic is thown down, which can yield ſufficient oxy- " gen to the zinc to combine with the remaining acid " undecompoſed. The precipitate which is formed, is " a mixture of metallic arſenic with arſeniate of zinc; " and theſe may be ſeparated by muriatic acid, which " will diſſolve the metallic ſalt, without acting upon the " arſenic.

" If, inſtead of muriatic acid, ſulphuric acid is uſed, " the ſame phænomena take place, in a leſs degree. " But, if the experiment is tried with nitric acid, there " is no diſengagement of hydrogen gas; and the metals " effect a precipitation with much leſs rapidity and cer- " tainty than in the former caſe.

" It is very true, that hydrogen, in its naſcent ſtate, " may have properties with which we are yet unac- " quainted; and may determine combinations, which it " can in no other ſtate produce. But the decompoſition " of water, in the firſt inſtance, in order that a new re- " compoſition may, at that moment, be effected by the " ſame agents, is in itſelf ſufficiently paradoxical."

Now, reader, what thinkeſt thou of this boaſted French theory: it would be an inſult to common ſenſe to make any comments: I leave thee to thy own reflection.

Mr. Chevenix alſo ſays (Nicholſon's Journal for De-

cember, 1802, p. 244), "In the analyſis of ſome menachanite from Botany Bay, given to me laſt year by the
" Preſident of the Royal Society, I obſerved, that while
" the oxyde of titanium was precipitated from the muriatic acid in which it was diſſolved, the exceſs of
" oxygen in the oxyde paſſed over to the muriatic acid
" and the potaſh, already in the liquor, and that hyperoxygenized muriate of potaſh was formed. I have
" attempted the ſame experiment with black oxyde of
" mangaueſe, but could not ſucceed.

"There is, however, a ſtill more extraordinary for-
" mation of this acid, in the diſtillation of nitro-muriatic
" acid upon platina. Oxygen is abſorbed by the metal;
" yet, not only oxygenized, but alſo hyper-oxygenized
" muriatic acid is formed. I have repeated the experi-
" ment ſeveral times; and am well convinced of the
" fact, however contrary to theory it may appear. I
" have tried the action of oxygenized muriatic acid
" upon nitric acid, in the hopes of forming hyper-oxygenized muriatic acid; but there was no action to this
" effect among their elements."

I alſo in this leave thee, reader, to thy own reflection, for I am tired with the province of arguing that *black* is not *white*.

But, Mr. Cavendiſh, I muſt whiſper into your ear; can you, as a man of common ſenſe, as a man of reaſon, as a man of ſcience, as a man of candour, or as a man of honour, defend any longer your *ſuppoſed boaſted diſcoveries*—that water is formed of oxygen and hydrogen, and the nitric acid of azote and oxygen—after this pamphlet? If you do, common ſenſe, reaſon, ſcience, candour, and honour, will call out ſhame! Let this French theory die its proper death, and let your own countryman have his proper reward, after all his unexampled treatment, and endeavour to atone to him for his unprecedented perſecution. Can it be ſuppoſed, in this ſcientific and civilized age, that, when a full and clear theory, fully elucidating all the phænomena of chemiſtry, (alſo with a number of ſtriking experiments, clearly elucidating the ſubject); will it be ſuppoſed by future ages, that ſuch unheard-of and unjuſt conduct could take place? That a combination could be ſo general, that neither the author, or his theories or experiments, ſhould be named; but both in part pilfered, as they found them neceſſary in eſtabliſhing their own abſurd opinions? Nay, in the other departments of philoſophy the ſame conduct has taken place. But the day of reckoning will come, and all the arts to ſuppreſs it will be ineffectual: then woe to its authors!

Hence clearly, according to their hypotheſis, as ſo great a quantity of the oxmuriatic gas was formed into the muriatic acid, then its oxygen gas ſhould have formed the ſulphur into the vitriolic acid. But ſo little acid does the ſulphurated muriatic acid poſſeſs, that Mr. Murray ſays "It converts vegetable blue papers to red; "but the change takes place ſlowly, unleſs the paper be "dipt into water! The paper is not corroded, unleſs "heat be applied." The water acts in the ſame manner it does upon the pyrities, in producing an action or fermentation upon the ſulphur and iron, decompounding the ſulphur of a part of its fixed fire, ſetting it looſe as free fire.

Then it appears, by treating ſulphur by thoſe different proceſſes, what different reſults they give, ſo oppoſite to their theory; ſo that I will make uſe of this aſſertion, and I call upon the combination to contradict it, *if they* *dare*—that, even in their elaboratory, in treating bodies by different proceſſes, what very contrary reſults they give; and that, when this theory is carried out of their elaboratory into the general phænomena of nature, nothing can be ſo *truly abſurd;* but, when examined by my ſyſtem, nothing can ſhew a more perfect agreement and coincidence. That I call upon them to ſhew, either in one of their experiments in the elaboratory, or one phænomena of nature, it does not clearly and beautifully account for: and, reader, *thou muſt ſay this*—that they ought to come fairly and openly forward, and ſhew, if they can, its deficiency; but they neither can nor dare; but act the part of dark aſſaſſins, with the Reviewers and other baſe agents, not even once mentioning my name, for fear of liberal ſcientific men, and the foreign chemiſts, getting hold of my publications, and inveſtigating them with candour.

Mr. Chevenix, in his paper in the Iriſh Philoſophical Tranſactions, ſhews the difference that the French chemiſts and he, from his experiments, makes the quantity of ſulphur that 100 of ſulphate of barytes contain:—the French chemiſts making it 23.43, while Mr. Chevenix makes it only 14.4 or 14.6.

Now it clearly appears that the ſulphur gives very different quantities of acid, according as it is formed by burning in oxygen gas, or by the action of the nitric acid upon it; and clearly explained upon this principle: that the nitric acid, upon robbing the ſulphur of its phlogiſton, or fixed fire, the ſulphur will act according to that principle in chemiſtry—if you rob a body of one of its conſtituent principles, it will attract another:—ſo the ſulphur, by being robbed of one of its conſtituents, attracts a part of the nitric acid; the other part ſeizes the phlogiſton, forming nitrous air. But when the ſulphur is burned in oxygen gas, it ſeizes the fixed air and water of the oxygen gas, and attracts them into its ſubſtance. But this aerial acid being a weaker acid, and only carrying with it water; therefore the vitriolic acid is found poſſeſſing leſs acid by being burned in the atmoſphere, than when formed by the nitric acid.[*] That water is formed when the ſulphur is burned in the drieſt oxygen gas, or the drieſt atmoſphere, appears clear; for a great quantity of water could be got from the acid procured this way. Then how could it get it, but from the gaſſes which went to their aerial compoſition? This is the true explanation of the very different reſults from the experiments of theſe chemiſts: the one making upwards of 38 per cent. of oxygen in the ſulphuric acid, and the other only 29. But the product of the vitriolic acid, from theſe different proceſſes, have led authors into the greateſt miſtakes. (See Dr. Thompſon's paper in Nicholſon's Journal,) what errors it leads them into. Indeed the pompous parade of experiments of our modern theoriſts is truly ridiculous. They are a maſs of errors.[†]

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[*] Mr. Chevenix makes the proportions 61.5 of ſulphur and 38.5 of oxygen. Mr. Thenard, whoſe proceſs was performed in the ſame way as Mr. Chevenix's, viz. by the nitric acid, makes ſulphur 55.56, and oxygen 44.44, while Mr. Lavoiſier makes 71 of ſulphur, and 29 of oxygen; but his proceſs was by burning the ſulphur in oxygen gas.

[†] It appears ſcarcely neceſſary here to mention Dr. Prieſtley's fooliſh hypotheſis, that fixed air is formed of hydrogen and oxygen; hydrogen, which is of an alkaleſcent nature, as ammonia, being principally formed of it. But more ſtriking, that in Volta's Pile the volatile alkali, and even the fixed alkali, are formed at the hydrogen wire. Therefore *a little appeal to common ſenſe:* hydrogen, the alkaline principle, can never form

It is juſt the ſame in vegetation, as I have fully ſhewn in my laſt publication, viz. my "Obſervations on the "Electrical Pile," &c. The fixed air, which, circulating in the vegetable, along with its juices, in the night, having no ſun-light, is diſcharged by the vegetable as fixed air; when in the light of the day, in the ſhade, as azote; but when the ſun acts with vigour upon the vegetable, it is diſcharged as oxygen gas, or pure air. Or even if water is ſaturated with fixed air, and expoſed to the ſun, the fixed air will be ſaturated with the ſun, ſo as to turn into pure oxygen gas, particularly if they are aided by an extraneous body, the threads of ſilk, if ever ſo long employed in the proceſs, or if ever ſo repeatedly waſhed in water: and therefore, even according to that prejudiced and violent defender of the French ſyſtem, Count Rumford, could not take any charcoal from the fixed air, as he afterwards proved.

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with oxygen the nitric acid, as he ſuppoſes. But I ſoon made our theoriſts fly from Doctor Prieſtley's abſurdities, and take refuge in the French chemiſtry. But let us examine the Doctor's theory by the fixed air turning to oxygen gas in thoſe experiments of his, juſt quoted. Now, though he has ſaid that the fixed air in metallic bodies is decompounded, the calces or oxydes of lead or mercury attracting the hydrogen, ſo as to be reduced, while the oxygen was ſet at liberty as gas: but then, how comes the fixed air to be in part changed into azote, and, nevertheleſs, the oxydes are reduced: nay the oxydes part with the fixed air, without its being decompounded. Here then muſt be an overplus of phlogiſton. From whence does it come? Alſo in the calcareous earths (ſee page 6) equally change part of their fixed air into azote and oxygen gas; but there are no oxydes to decompound it; and the lime that is formed poſſeſſes no phlogiſton. But it is too ridiculous to refute. Theſe obſervations occurred from the experiments juſt quoted; but its abſurdity I have fully ſhewn.—(See my former works.)

Nay, according to Dr. Woodhouſe, charcoal forms fixed air into as pure oxygen gas as vegetables.

On ſaturating the pureſt diſtilled water with fixed air, and then freezing it, upon the latent heat of the water being ſet looſe, it partly united to ſome of the fixed air, and formed with it azote. Alſo Dr. Prieſtley and Mr. Cavendiſh found that by ſaturating water with fixed air, and then expelling it by heat, part of it was turned into azote; and alſo Dr. Prieſtley ſays, (vol. iii. p. 238, ſpeaking of what they call the gaſeous oxyde of azote, or the nitrous oxyde, or what Dr. Prieſtley calls the dephlogiſticated nitrous air—but, in ſhort, our *learned theoriſts* multiply names ſo, that it is not known where they will ſtop)—" No leſs remarkable was the reſult of " heating malleable iron in this air," (meaning dephlogiſticated nitrous air.) " For the bulk of the air was " increaſed, and yet afterwards no part of it was imbibed by water. It was all phlogiſticated air, without " any mixture of fixed air. It ſeems, therefore, as if " this kind of air wanted nothing but phlogiſton to make " it phlogiſticated air, and nothing but the principle of " heat, to make it dephlogiſticated air.

" This experiment happening to draw my particular " attention, I repeated it very often, and always had the " ſame reſult, though with variations, depending on the " quantity of the air, and the heat I was able to apply."

Here, clearly, this air, which is formed of the nitrous acid and phlogiſton, by the imperfect combuſtion of the malleable iron, and by the action of heat, they are both more aerialized, and the acid and phlogiſton more firmly united, ſo as to form a kind of azote, but very different from the azote of common air; juſt as the nitrous oxyde is very different from oxygen gas, both being completely noxious to reſpiration, though the nitrous oxyde will allow bodies to burn in it with an enlarged flame.—But Dr. Prieſtley ſometimes formed it with ſuch a large proportion of phlogiſton, as to explode all at once*. But I will quote ſome experiments made by Dr. Woodhouſe,

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[*] Alſo Dr. Prieſtley ſays, (vol. iii. p. 281.) "However, by heating "iron in fixed air, there can be no doubt but that a ſenſible quantity of "it is converted into phlogiſticated air; which agrees with the experi-"ments that I formerly made, by putting pots of iron filings and brim-"ſtone into fixed air. The experiments that I made of this kind were "the following, in which it will be obſerved, that, though in ſome of "them, there was an increaſe in the quantity of air after the proceſs, "yet that it was by no means equal to the quantity that remained un-"abſorbed by the water; and therefore there muſt have been a farther "addition made of this kind of air in the proceſs.

"After heating turnings of malleable iron in a quantity of fixed air "for ſome time, I examined a part of it, and found that about one-tenth "of the whole was immiſcible with water. Having reſumed the pro-"ceſs with the remainder, I found a reſiduum of one-fourth of the "whole. There ſeemed to be a ſmall addition to the quantity of air, "after the firſt part of the proceſs, but I could not perceive that there "was any after the ſecond. I reſumed the proceſs a third time, but did "not find that I had made more than one-fourth of the whole immiſ-"cible with water. At another time I heated the ſame kind of iron in "fixed air, till of 3¾ ounce meaſures of air there was a reſiduum of 0.8 "of a meaſure, which was ſlightly inflammable, burning with a blue "flame; and in this caſe there was no ſenſible addition to the quantity "of air at all. Laſtly, I heated iron in three ounce meaſures of fixed "air, till there was an addition of 0.4 of a meaſure to the quantity of "it; but there was a reſiduum of one and a half of a meaſure not ab-"ſorbed by water, which burned with a ſlightly exploſive blue flame."

This is their heavy gaſeous oxyde of carbon, which appears to be formed in many proceſſes where fixed air diſſolves or unites to phlogiſton, either as hydrogen gas or fixed in metals, or in many other ſtates, as I have found upon the ſame principle that Mr. Henry found that vinegar would diſſolve the eſſential oils.

on air being purified by being expoſed to the light of the ſun, having different bodies placed in the water.—(Nicholſon's Journal for July, 1802, p. 153.) "This water "was taken from a well ſunk within a few yards of a "neceſſary, from which it was impregnated with carbonic acid gas, as appeared from an analyſis. The "leaves were ſeparately expoſed in glaſſes arranged near "each other, and from eight to thirteen comparative experiments were made at one time.

"Filaments of aſbeſtos, baked horſe hair, common "cotton, and that of the aſclepias Syriaca, the flower "panicles of rhus cotinus, the fine hairy plumes of "climatis criſpa, the ſpikes of panicum glaucum, and "charcoal in powder. From each of theſe ſubſtances, "from two to four drachm meaſures of pure air were "obtained, which devoured nearly two meaſures of "nitrous air."

Now here it appears from the French theory, that charcoal muſt have decompounded the fixed air of the water, taking charcoal from it. Will my reader, will common ſenſe, candour and juſtice, require me to refute ſuch abſurdities, as they are obliged, in defence of this French theory, to adopt? No; I will not pay his heart and head ſo bad a compliment—it is equally clear in the analytical as in the ſynthetical. In imperfect combuſteous reſpirations and putrefactions, (in my "Experiments "and Obſervations on the Galvanic Pile," p. 30) I ſay, 'That azote is an air, with a leſs ſaturation of fixed fire 'than pure air, I have fully ſhewn in my former publi'cations, that in all proceſſes where pure air is injured 'ſlightly, it is turned to azote, as in imperfect combuſti'ons, reſpirations, or putrefactions. Dr. Prieſtley has 'lately publiſhed ſome experiments which ſhew it. In 'throwing the rays of the ſun by a mirror upon bones, 'needles, &c. there was not a free combuſtion, but an 'imperfect one, ſince he clearly burned a great quantity 'of pure air into what he calls phlogiſticated air. It is 'alſo ſhewn by an experiment of Mr. Metherie's, who 'plunged burning charcoal, previouſly extinguiſhed in 'mercury, into the pureſt oxygen gas, which it gradually 'imbibed. Afterwards, by plunging it into water, one 'fourth of the air was diſengaged from the charcoal, 'which principally conſiſted of azote. Now this expe'riment can only be explained by the heated charcoal 'making an imperfect combuſtion: for if a great quan'tity of its fire had not been taken from it by the 'mercury, it would have burned the air into fixed air. 'The experiment is not contradicted but by ſaying that 'charcoal forms with pure fixed air, and therefore 'cannot form azote. Here they contradict facts by their 'hypotheſis alone, as if it was the immutable truth.

'By confining phoſphorus in pure or atmoſpheric 'air, I have made it burn with a different degree of com'buſtion, according as the glaſs which incloſes them is 'expoſed to the different degrees of heat; ſometimes 'making it burn ſo freely as to imbibe the pure air, 'and ſometimes only to turn it into fixed air, ſometimes 'only into azote, and ſometimes both into fixed air and 'azote. Theſe ſimple and clear experiments ſpeak ten 'thouſand times more forcibly than all the complicated 'experiments, which are only calculated to embarraſs 'truth. I have ſometimes thought that a drop of water 'aided theſe proceſſes.'*—Alſo Mr. Bockman, in the Philoſophical Magazine for June, 1803, p. 21. "I filled a bell "glaſs with oxygen gas prepared from oxyde of manga-"neſe well waſhed with milk of lime. The wood "immerſed in it did not emit a ſtronger light than in "atmoſpheric air or water. At the end of forty-eight "hours the light ſeemed to decreaſe a little, and on the "ſixth day it was about a third weaker. The phoſphor-"eſcence afterwards ſlowly decreaſed; on the 14th day "it ceaſed entirely, and was not afterwards revived in "the open air. The volume of the gas decreaſed very "little, ſcarcely 0.2. Having put another piece of wood

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* Having ſhewn the fallacy of their arguments in reſpect to the compoſition of fixed air, when ſpeaking of pure air being formed by

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expoſing pure water impregnated with fixed air to the rays of the ſun; I conclude in this jocular ſtrain: 'Now, can Mr. Lavoiſier, upon 'the formation of pure air from fixed air, find the carbone, which 'ought to have been depoſited in the water, being ſet free from its 'combination with fixed air? Nay, will it not give our reader a 'laughable ſurpriſe when I tell him, that Mr. Lavoiſier ſeriouſly 'propoſes a manufactory to obtain charcoal by the decompoſition of 'fixed air? See his Experiments, p 230. But I will hint to him a 'better manufactory, and one more conformable to his hypotheſis. 'He ſays that water conſiſts of hydrogen and oxygen gaſſes, with the 'addition of carbone, or charcoal, form alkohol or ſpirits. Now, as 'the river *Seine* produces plenty of water, and as charcoal is a cheap 'commodity, the tranſmutation of water into ſpirits would be a ma-'nufactory that would turn to good account. This would lower the 'price of *French* brandy in Old England; or, as ſeveral of our Engliſh 'chemiſts are no leſs induſtrious and ingenious than Mr. Lavoiſier, 'the Thames might be turned into good *Britiſh* ſpirits; which would 'render that article ſtill cheaper. But, alas! this, I am afraid, will 'ſtill be one of the chemical deſiderata; and, as this kind of chemiſtry 'will not effect ſo much good, an alarm may be ſpread on the other 'hand. For, according to them, water is formed of inflammable and 'oxygen gaſſes, two bodies the moſt combuſtible in nature. If, there-'fore, they ſhould be able to ſet the Thames on fire, London would 'be in danger of being reduced to aſhes.'

"into the remaining gas, it continued to emit light "without being weakened.

"On trying the gas which remained in an eudiometer, it ſhewed only a decreaſe of 21 degrees; and in "about 15 ſeconds, when the red vapour of the nitrous "acid diſappeared, I obſerved a white faintiſh vapour "from the gas floating over the water. For the ſake of "compariſon I tried a portion of the ſame oxygen gas "in a ſimilar bell, cloſed in the ſame manner with water, "in which no wood had been immerſed, and found "that in the ſame eudiometer it ſhewed a diminution of "266 degrees. Artificial phoſphorus, when placed in "the remaining gas, became luminous and evaporated. "A taper immerſed in it was immediately extinguiſhed: "it was not inflammable, had no conſiderable ſmell of "mouldineſs, and rendered lime water turbid; but it "was not abſorbed by it in any conſiderable degree.

"Having repeated this experiment ſeveral times, "I obtained ſimilar reſults, or reſults very little different."*

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* '*Carliſle, June* 12, 1801.—Mr. Urban, I have ſeen in Mr. Nicholſon's 'Journal a paper of Mr. Cruikſhanks, in oppoſition to ſome of Dr. 'Prieſtley's experiments and obſervations; which paper, all the French 'votaries and journals exultingly ſay, confirms the antiphlogiſtic doctrine: but I hope to turn their joy into *weeping and gnaſhing of teeth.* 'I beg leave to make this general remark, viz. both their hypotheſes 'are abſurd, conſequently open to cenſure, each detecting glaring abſurdities in each other's doctrines. I have examined and compared 'their doctrines with all the different phenomena of reſpiration, vegetation, putrefaction, Signior Volta's Galvanic pile, &c. &c. (the laſt 'work is juſt now publiſhed by Mr. Cadell); and, I flatter myſelf, I 'have ſhewn the beauty of my hypotheſis and the deformity of theirs, 'in a clear and ſtriking manner, by juſtly accounting for all theſe 'phenomena. I ſhall now enter upon the examination of Mr. Cruik-

Let us conſider the grounds on which our modern philoſophers have formed their opinion, that carbone

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'ſhank's paper, from which I hope to deduce the very ſame concluſions.

'Fixed fire, an acid, and water, in a ſtate of neutralization, form pure air; a metal is formed of its calx and a full ſaturation of fixed fire. When the pure air and the metal are burned together, the fire of the pure air and that of the metal are ſet looſe, and become free fire, the acid and the water of the pure air entering the calx or earth of the metal. This is the phenomena of the calcination of metals with pure air. But, Mr. Urban, there is another method of calcining metals. Iron (for inſtance) is calcined by the ſteam of pure water, the iron being red hot. Now, in this caſe, the iron and its fixed fire both attract the pure water, and the fixed fire forms with it inflammable air. Yet theſe calces are very different, the one poſſeſſing both an acid and water, and the other only water. That they are different is proved from this: the calces made in pure air are conſiderably lighter than the calces made by the ſteam of water: moſt calces in the former proceſs only gaining one-tenth, but of the latter one third of their weight by the calcination. And for this reaſon, (as I have fully illuſtrated in my letter to Mr. Cavendiſh, ſee p. 96), the acid being a ſtronger body than water, a leſs weight of it ſaturates the calx, which requires more water than acid.

'The French philoſophers ſay, that a calx is formed by the metal imbibing pure air; and, according to Dr. Prieſtley, it is formed by the metal imbibing pure water. By expoſing the calces to phlogiſtic bodies, let us ſee the phenomena which may be juſtly deduced from Mr. Cruikſhanks's experiments. When the grey calx of iron from a ſmith's forge (and therefore calcined by the pure air of the atmoſphere) was heated with charcoal, the calx was reduced, and both an acid air, viz. fixed air, and a heavy inflammable air, were produced. Now, according to Mr. Lavoiſier's ſyſtem, the charcoal acts in reducing the calx, by attracting the pure air from the calx, and phlogiſton has nothing to do in the proceſs. See his work tranſlated by Mr. Henry, in which he endeavours to prove, that fixed air only is produced; and his experiments to prove this were ſome of his firſt and greateſt, which led him to form his ſyſtem. But it appears, both from Dr. Prieſtley and oxygen form fixed air. Mr. Lavoiſier, upon burning charcoal in oxygen gas, found, he ſays, that the fixed

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'and Mr. Cruikſhanks, that an inflammable air is alſo produced, and in much greater abundance than the fixed or acid air, viz. five to one. But I ſay, that, according to Mr. Lavoiſier's doctrine, there ſhould not be one particle of this inflammable air produced, as pure air and charcoal produce only fixed air; and, according to them, there is nothing ſave pure air and charcoal in this proceſs. For there could be no extraneous water, as the charcoal and the calx were expoſed ſeparately to the ſtrongeſt fire.*

'That the calces, when the charcoal gave them their fixed fire, parted with ſomething more than water, appears from this, that the inflammable air being different from the inflammable air which is obtained by moiſtening charcoal with water, it being ſo much heavier. However, that air which is produced by paſſing fixed air and water through phlogiſtic bodies, is identically the ſame air, appears clearly from Mr. Cruikſhanks's experiments. By heating chalk (which is well known to conſiſt of an earth united with fixed air and water) along with iron, it is evident that the fire expelled the fixed air and water from the earth of the chalk, forming it into a lime (its well-known effect) and they entering the iron, take from it its fixed fire, and form together this heavy inflammable air of Mr. Cruikſhanks: It is clearly ſo, for if water alone had paſſed through the iron, a light inflammable air would have been formed.†

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* 'Nature exiſts by combinations; when, therefore, the calx of iron attracts the fixed fire of the charcoal, it again parts with the acid and water it had received in its calcination. Its earth having a ſtrong attraction for theſe bodies, therefore its being ſeparated from the one muſt be by attracting the other.'

† 'Here the iron is calcined by the ſame bodies as when it is calcined by burning in the atmoſphere; by the action of fire, fixed air and water, the fixed air and water in both caſes entering the calx; only in one caſe the fixed fire is ſeparated from the iron as free fire, and in the other it is ſaparated as fixed fire, viz. inflammable air. Iron requires for its calcinationn both an acid and water. But if the water is aided by the heat, as ſteam, it will of itſelf calcine iron; the vitriolic acid alſo will, without the water, calcine iron, if aided by heat. The water, as ſteam, acts in the very ſame way as the water and fixed air in the chalk, one part of it entering the calx, and the other part forming inflammable

air produced was equal to both the oxygen gas and charcoal: but, indeed, moſt of this gentleman's experiments

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‘In moſt proceſſes of combuſtion the pure air is left in the ſtate of fixed air; but in theſe calcinations it is imbibed by the calces. Now the fixed air, which is a heavy air, paſſing with the water and attracting the fixed fire of the metal, would make the inflammable air ſo much heavier, is what muſt be the caſe. It is alſo thus ſtrikingly confirmed; for when this inflammable air is burned with pure air. the quantity of fixed air produced is above double the quantity of pure air employed: alſo from its requiring ſo little pure air to burn it, being formed of fixed air which is incombuſtible, and which fixed air being mixed with inflammable air muſt make it heavy, as fixed air is heavier than even pure air.

‘Let us now attend to Mr. Cruikſhanks's explanation of the phenomena. He ſays, this heavy inflammable air is formed of 21 parts of pure air, and nine parts of carbone. Carbone and pure air were always ſuppoſed by them to form fixed air; but here, they tell us, is a new air produced for the inveſtigation of our *wiſe aerial philoſophers*. This air, Mr. C. ſays, is produced only by thoſe calces which can bear a red heat. And, as the calces are reduced in the proceſs, it muſt be by the charcoal under this red heat attracting the pure air ſtronger than the calces. But, more extraordinary ſtill; the fixed air is principally produced in the firſt part of the proceſs, when the heat is not ſo intenſe*; and as heat is neceſſary to burn charcoal and pure air, therefore, if reaſon be allowed a hearing in theſe controverſies, the greater the heat, the more complete the burning, or union of the carbone and pure air; and *therefore ſhould form only* fixed air. But this heavy inflammable air appears in another proceſs, with reſpect to which they adopt, *if poſſible*, a ſtill more abſurd and prepoſterous explanation. When chalk and filings of iron produce

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air. In the ſame manner does the vitriolic acid act; a part of it enters the calx, forming it, while another part enters the air forming the vitriolic acid air, an experiment chemiſts are well acquainted with. Nothing can be more ſtriking than this beautiful concatenation of facts, very different from the abſurdities of the French chemiſts”

* ‘For a full view and explanation of the phenomena, which attend the action of the calces of metal and charcoal upon each other, ſee my Chemical Eſſays, p. 46.

have been defective. Mr. Cruikſhanks ſays, in Nicholſon's Journal, that the fixed air is not equal in weight.

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it, he ſays, it is from the fixed air being decompounded by the iron attracting part of its oxygen from it. Need I aſk Mr. C can he decompound fixed air by heating it in the ſtrongeſt heat with red-hot iron? I obſerve in their explanation of the phenomena, that they will rather adopt the greateſt abſurdity than give up their favourite hypotheſis. Away with ſuch abſurdities! it is vexing to have ſuch puerilities to combat[†].

'*If Mr. Cruikſhanks will take the calx of iron made by the ſteam of pure water, and heat it with charcoal, though previouſly expoſing them ſeparately to a red heat, he will produce a lighter inflammable air, which he calls hydrocarbonate. Or another experiment equally as deciſive. If he will take this heavy inflammable air, which, he ſays, is formed of 21 parts of pure air and of nine parts of carbone, and reduce by it the calx of iron formed by the ſteam of water, he will find in the reduction a great quantity of water in the proceſs; and the inflammable air well be imbibed by the calx. But according to our bright aerial philoſophers, all this heavy inflammable air ought to have been formed into fixed air, it receiving from the calx the addition of pure air it wanted to form fixed air; and not an atom of water ſhould have been found in the proceſs*[‡].

'This is ſuppoſed to be the age of *aerial* philoſophy; I wiſh it were the age of common ſenſe, for at preſent it has taken an aerial flight; and, unfortunately, candour and juſtice have flown away with it. *O tempora! O philoſophia!* 'ROBT. HARRINGTON.'

[†] 'Here is an inflammable air which will explode with pure air; yet they can ſay with great gravity there is no inflammable air in it, it being formed of pure air and carbone. *Wonderful!* Heads are formed of brains. But I can with greater propriety ſay that ſome heads have no brains in them, than they can ſay that this combuſtible air has no inflammable air in it.'

[‡] 'That fixed air will be united with metals in their metallic ſtate, is evident: for I have proved (ſee my Eſſays), that if minium is newly made (and not expoſed to the atmoſphere to imbibe moiſture) it will be reduced by the heat alone: its fixed air not having water to aid the union of the fire and fixed air, there will no air come from it at its reduction; but the fixed air will remain in the metal. The grey calx of lead will by heat give out fixed air; but when the calx is expoſed to a reverberating furnace, ſo as to receive fire ſufficient to form minium, the fixed air will receive ſo much fire in the proceſs as to form it all into pure air, which it will give out by heat. Here the fixed air is evidently changed into pure air by the fire ſaturating it.'

But only attend to what the immortal Scheele ſays*: his authority I think, in point of judgment, ſuperior to all our moderns. Nay, only attend to my fact, which Dr. Prieſtley has now fixed Mr Cruikſhanks upon, and which his ſilence acknowledges to be true; viz. that, in reducing the calx of iron, formed by the ſteam of water, with charcoal, that in the proceſs both fixed air, and the pureſt, what they call hydro-carbonated gas, is only produced. Now as I have clearly ſhewn (ſee my Letter) that there is no decompoſition of water in the proceſs, it merely analyzes the bodies which compoſe the charcoal; viz. a vegetable acid, and a vegetable ſulphur, as Scheele calls it; or, what is more proper, fixed fire united to the vegetable ſubſtance or acid. When vegetables vegetate by the action of the ſun's rays and heat, they attract their fixed fire, and I think, without doubt, (ſee my former works) that they decompound the water, receiving from it part of their earthy ſubſtance: for, as Van Helmont, M. Jellet, Dr. Crell, and myſelf, have clearly ſhewn that vegetables will vegetate to perfection in the pureſt water. As to Mr. Haſſenfratz's experiments, he did not take the ſtrongeſt vegetables for the purpoſe, though they did not come to perfection, and yet, clearly, in that ſtate they contain more carbone than they ſaid. But Mr. Tillet's and Dr. Crell's experiments are directly demonſtrative: and even as to the French experimenters, in ſupport of their theory, they are always to be diſputed.

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* 'That charcoal contains aerial acid as well as inflammable air appears from an experiment of Mr. Scheele's, p. 182.—" The charcoal is " moſt proper for the purpoſe, ſince it is a ſulphur compounded of phlogiſton and aerial acid. If coals be ground together with alkali, made " cauſtic by quick lime or fire, a great quantity of inflammable air is thus " obtained, containing no aerial acid. If a bladder be tied to the mouth, " the alkali, on the other hand, loſes its cauſticity, and effervesces with " acid."

In this controverſy with Dr. Prieſtley and Mr. Cruikſhanks, in the Doctor's laſt paper, he entirely adopts my ſtatement, confining his adverſary to the calx of iron formed by ſteam (though he meanly ſuppreſſes my name) which the other is unable to anſwer; and the Doctor now meanly ſhuffles away his former abſurd opinions. The French and Dutch chemiſts have had equally as abſurd a controverſy, the particulars of which I do not think it neceſſary here to name. They have been contradicting each other's moſt obvious and clear facts, to force the phenomena to their fooliſh hypotheſes, or men of ſtraw, adopting the moſt glaring, puerile, and abſurd opinions.

Theſe facts are clearly gathered from their experiments: viz. that water paſſing through charcoal, forms what they call hydrocarbonate air; that dry fixed air paſſing through charcoal, forms a very heavy carbonated air; but that fixed air and water paſſing through charcoal, form an air a medium betwixt theſe airs (and which they call gaſeous oxyde of carbone); and that pure air paſſing through charcoal, only forms fixed air. Now, need I explain to my reader the clear and obvious concluſions from theſe experiments. Metals that were calcined by the air of the atmoſphere, upon being revived by the fixed fire of the charcoal, parted with the fixed air and water they had received from the atmoſphere, and they paſſed through the charcoal, and formed this gaſeous oxyde of carbone. But when the iron that is formed by ſteam is revived, it parts with its water, which paſſes through the charcoal, forming the hydrocarbonated air.*

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* But let us ſee what the foreign chemiſts ſay of the phenomena. The Critical Review, or Appendix to Vol. XXXV. p. 567, ſays, "Mr. Woodhouſe has publiſhed ſome obſervations on the objections of Dr. Prieſtley to the antiphlogiſtic ſyſtem in the Medical Repoſitory, an American collection. Theſe, and ſome remarks in a ſeparate publication, are abridged in the preſent volume; and, on the whole, we perceive that this chemiſt, though in favour of the doctrine of Lavoiſier, finds ſome of Dr. Prieſtley's objections correct. In theſe experiments, however, viz. in that where zinc was reduced by carbone,—a new gas was diſcovered, which burns when lighted, in common ditonates, though feebly, with oxygenous gas, and requires a large quantity of it. It is lighter than carbonic acid gas, and heavier than carbonated hydrogen — the weight being nearly that of atmoſpheric air. When iflamed over oil or mercury it affords no water, and it then leaves carbonic acid air in perfect purity. The ſame gas is obtained by treating oxyde of zinc with plumbago, and expoſing carbonate of barytes with pulveriſed charcoal to the fire of charcoal in a porcelain tube, heated red, will produce the ſame air if carbonic acid gas be repeatedly paſſed through it. This is therefore an oxydated gas of carbone.

"To prevent any interruption, we ſhall purſue the ſame ſubject as treated in the 39th volume.

"Mr. Guyton read to the National Inſtitute a memoir on the cumbuſtion of oxydated gas of carbone without heat. He found that the carbone was in a very different ſtate from that in which it forms the carbonic acid and hydrogenated carbonic gas, and is not in a condition to act in a ſolution of metals the moſt eaſily reducible. It may, he found, be in part burned by the oxygen of the oxygenated muriatic acid, and then acquires all the properties of the carbonic acid; but this combuſtion operates only gradually, as if the affinity were determined by the maſs of oxygen in action, and in reality it is only reduced to carbonic acid by operating repeatedly on the ſame gas. M. M. Deſorms and Clement have, however, elucidated the ſubject more completely, in a memoir on the reduction of the white oxyde of zinc by charcoal, and on the oxydated gas of carbone which reſults

That the reader may be clearly aſcertained of theſe obvious and clear facts, he may be fully ſenſible of be the following experiment.

“ from it. Their experiments are varied and minute, but we cannot follow them cloſely. They have clearly proved the exiſtence of an aerial oxyde of carbone, whoſe proportions of carbone vary from 45 to 52 parts in 100, according to the quantity of charcoal employed, and the temperature at which the mixture is made. It is ſingular that this gas cannot be produced directly by uniting the given quantities of carbone and oxygen, and that it is only formed by adding carbone to carbonic acid. They in vain attempted to form it by ſuffering oxygen to paſs ſlowly over red hot charcoal, the reſult was only carbonic acid, unleſs it remained long in contact with the carbone.”

As I live in a provincial town, far from ſcience, where I may properly ſay I am inſulated, therefore it is impoſſible for me to have acceſs to the different publications; ſo that I catch parts where I can. Now, only to reflect upon theſe experiments:—

Although moſt of the calces of metals are very difficult to ſeparate their air by the action of heat alone, we can always do it by charcoal; therefore, examining the air that comes from them will ſufficiently ſhew what kind of air they poſſeſs, whether fixed air or oxygen gas. As oxygen paſſing through charcoal, the French chemiſts, by all the arts they were capable of, could not produce any air but fixed air, as we might have argued *a priori*, as in the proceſs there would be a combuſtion, and every tyro knows that they only produce fixed air in combuſtion.

But if fixed air is paſſed through the hot charcoal, he knows equally well that they will produce no combuſtion. But he will likewiſe know that water, when paſſed through the hot charcoal, takes a part of the charcoal along with it, and gives the charcoal its water of compoſition, forming what they call hydro-carbonated air. Then, when fixed air paſſes through an aerial body, he will naturally ſuppoſe part of the charcoal will likewiſe be taken or united with it; and they form a heavy air, ſo as to be united to 46 or 52 parts of carbone. Now, here are three airs formed by paſſing through hot charcoal, and their quality and weight directly correſponding to what we might expect *a priori*. And what muſt Mr. Cruikſhanks think and ſay, if he has any candour? That barytes

Take two large flat tin veſſels, their ſides riſing high in the middle, ſo that by compreſſing their ſides you will move whatever is contained within them. Fill theſe veſſels with pure dry fixed air, and connect them with a hollow iron cylinder, which contains pure hot charcoal, and place this cylinder over the flame of a lamp: then, by firſt compreſſing the ſides of one of the veſſels, and then the other, you will make the fixed air repeatedly paſs through the charcoal; and if there are two rings fixed to the centre of each of the ſides of the veſſel, it will aid the proceſs, by pulling them up when flattened. When examining the fixed air after the operation, it will

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and pure fixed air produce the ſame air as he found the calces of zinc, iron, &c. produce. But ſtill, to corroborate my theory of the calcination of metals, the calces of zinc, iron, &c. have alſo water in their compoſition, and therefore, when they receive the fixed fire from the charcoal neceſſary to reduce them, they part with both their fixed air and water, and which, together with the hot charcoal, produce an air not ſo heavy as that which is produced by the dry fixed air alone, and not ſo light as that which is produced by water alone, their weight and quality being a medium of both—that is, partaking of both: and to prove to demonſtration the truth, theſe calces, as the precipitate of mercury per ſe, only give fixed air with the charcoal. (See Mr. Cruikſhanks's experiments.)[*] Mr. Cruikſhanks performed theſe experiments with bladders moiſtened with water, in order, he ſays, to cool the end of the iron they were fixed to; but would not the moiſture penetrate the bladders, *you profound philoſophers?*

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[*] In the preſent theory, every thing muſt bend to it. Would any rational chemiſt ſuppoſe, nay any man of common ſenſe, that when fixed air is formed by burning charcoal with oxygen gas, that by expoſing this fixed air again to hot charcoal, that the reverſe operation could take place? — that the charcoal could take oxygen from that charcoal it was ſaturated with by the *very ſame operation*, viz. oxygen and charcoal acting upon each other by combuſtion? What ſolid chemical knowledge could lead to ſuch abſurdities, before we got into our aerial Gallic flights? And our Engliſh chemiſts ſtand gaping, echoing their abſurdities.

be found to be ſtrongly impregnated with this very heavy carbonated air. If fixed air, highly moiſtened with water, be put into the tin veſſels, and paſſed through the hot charcoal, it would be found to be the gaſeous oxyde of carbone; and if pure water be forced through the charcoal, every one knows it will form only hydro-carbonated air.

Now I call upon chemiſts to perform theſe experiments, and to come fairly forward and acknowledge the reſult. I particularly call upon Dr. Woodhouſe, whom I think a man of candour. Need I mention to my reader Mr. Cruikſhanks's explanation, that the fixed air having a part of its ſuppoſed oxygen taken from it, forms the gaſeous oxyde of carbone. That when metals are revived, by charcoal in this proceſs, that the pure air the calx or oxydes contain paſſing through the charcoal, the oxygen gas, inſtead of forming fixed air, which it always does with charcoal, receives a larger proportion of charcoal, and then forms this air. But, then, by all the trials that theſe experimenters have been able to make in paſſing oxygen gas through charcoal, they are obliged to confeſs they can only form fixed air. But Mr. Cruikſhanks, in expoſing iron and chalk to great heat, produced this air, which he ſays is performed in this way: the fixed air of the chalk is robbed of part of its oxygen by the iron; but, unfortunately, he *did not know* this plain and obvious fact, that chalk contains, by chemical attraction, water as well as fixed air: but let him force dry fixed air through heated iron, and he will not be able to form the gaſeous oxyde of carbone. We ſhould not be ſurpriſed at fixed air attracting a part of the charcoal, as even ſulphur will do it, forming with it an air.

Dr. Prieſtley ſays, (Nicholſon's Journal for September, 1802) " Mr. Cruikſhanks ſays, p. 201, that " the " oxygen in the finery cinder, uniting with the car- " bone in the charcoal, forms fixed air; and that the " metal being in the ſame proceſs revived, decompoſes " this fixed air, when it becomes again to a certain de- " gree oxygenated." But why ſhould not the finery " cinder retain a part of its oxygen, rather than firſt part " with it, and then take it again? Beſides, it is not true " that after this proceſs the iron is in any degree oxy- " genated, for it is completely revived, being perfect " iron; and that any fixed air is either formed or de- " compoſed in this proceſs is altogether conjectural, and " for the reaſons that I have given cannot be admitted. " For though it might be poſſible for oxygen in the finery " cinder (ſuppoſing it to contain that principle) to be " extracted from it by its affinity with the carbone in " the charcoal, and that nothing ſhould enter in its place, " the iron thus revived could not decompoſe the fixed " air that would be formed by their union.

" Not doubting but that Mr. Cruikſhanks will give a " candid attention to theſe obſervations."

Need I make any comment upon theſe abſurdities? Mr. Cruikſhanks mentions equal abſurdities that the Doctor's hypotheſis leads to: ſo theſe men go on, contro-verting and knocking down each other's men of ſtraw.

Mr. Cruikſhanks ſays, (Nicholſon's Journal for May, 1802, p. 42) " In your Journal publiſhed the 1ſt of this " month (March), I find a letter from Dr. Prieſtley, in " which he ſtill defends the old doctrine of phlogiſton, " but with very little ſucceſs; for, having additional " difficulties to ſtruggle with, he has been under the " neceſſity of adopting new, and ſometimes contradictory " opinions, in his explanations and defence; for ex-

"ample, it is now his opinion that all metallic calces "contain water and little or nothing elſe; and that charcoal, uniting with water, forms both fixed and inflammable air; for as this ſubſtance contains the elements "of both kinds, nothing but water is wanted to enable "them to take the form of air. It is almoſt impoſſible "to argue againſt ſuch ſtrange ſuppoſitions and looſe "reaſonings as theſe." Here he equally knocks down, by fair arguments (ſee the Journal), the Doctor's hypotheſis; ſo the public *muſt diſcover* that both their hypotheſes are equally vulnerable and abſurd. Mr. Cruikſhanks, in this paper, ſays, (p. 47) "There are "ſome circumſtances accompanying the diſtillation of "charcoal with theſe metallic oxydes, which are eaſily "reduced, and at low temperatures, not altogether uninſtructive, and, in my opinion, unanſwerable by the "ſupporters of phlogiſton. In theſe proceſſes the proportion of gaſeous oxyde to carbonic acid gas is but "ſmall, being in general about one-third or one-fifth of "the whole, and for the moſt part is obtained juſt before "or at the time the mixture becomes red: it is always "accompanied with a torrent of carbonic acid gas. At "the inſtant the metal is revived, the gas either ceaſes "entirely, or comes over very ſlowly; but on increaſing "the heat, it again makes its appearance, and is now "ſo far from containing gaſeous oxyde, that it is peculiarly light, not mixed with any ſenſible quantity of "carbonic acid gas, and yields, when ſaturated with "oxygen, but a very ſmall proportion of this acid gas.

But, let us conſider theſe unanſwerable arguments of Mr. Cruikſhanks. Though Dr. Prieſtley is not able to anſwer them according to his hypotheſis, let us try them by Mr. Cruikſhanks's and mine.

Now, I acknowledge in theſe oxydes that have got their fixed air neutralized with fixed fire, that, in conſequence of being heated, they give out oxygen gas; and that, when the gas enters the charcoal, a combuſtion takes place, and fixed air is principally formed; juſt the ſame as when the French chemiſts paſſed oxygen gas through the charcoal, they could not, by all the trials they made, produce any of this gaſeous oxyde, but only fixed air, and they were ſurpriſed. But as they ſay all oxydes contain pure oxygen, as well as the oxydes of mercury, why do they not alſo give out oxygen gas with charcoal? For this clear and obvious reaſon — they contain no oxygen, but only fixed air. And my doctrine ſtill appears clearer: for I have ſufficiently proved, that in the oxydes of mercury, precipitate per ſe, and minium, that their fixed air, as it becomes impregnated with fixed fire, will become red; and Mr. Cruikſhanks ſays, that the gaſeous oxyde which is obtained in this proceſs, is juſt before the mixture becomes red[*], that is, juſt before all the fixed air of the calx of mercury becomes fully ſaturated with fixed fire, ſo as to become oxygen gas.

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[*] I have long wondered at our French theoriſts (but now all wonder ceaſes)—at their contradiction and abſurdities. Mr. Cruikſhanks ſays, that the oxydes require a red heat to produce this gaſeous oxyde of carbone: and yet, in this very experiment, which he ſays it is impoſſible for the phlogiſtians to anſwer, "It is obtained juſt before the mixture becomes red;" that in other oxydes (iron, zinc, &c.) they require a ſtrong heat, no doubt, to expel the fixed air and water they are united to. But mercury is well known to retain its airs very ſlightly. But that it evidently appears from his own experiments, that the forming this gaſeous oxyde of carbone is not from the heat required to decompound the fixed air into the gaſeous oxyde, as he ſuppoſes; for here it appears before the mercury comes to a red heat; and when it comes to a red

About the time that the metal is revived, there is little gas produced. Now, upon theſe oxydes being reduced, they had given out all their oxygen, and in giving out all their oxygen combuſtion had taken place, in conſe-

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heat, only fixed air is produced; therefore it muſt *evidently* be from the cauſe which I aſſign, viz. from the mercury not having its fixed air neutralized by fire[*]. — Mr. Cruikſhanks ſhould have been very careful in arraigning Dr. Prieſtley's experiment in expoſing fixed air to heat. He ſays (p. 46), "The method by which Dr. Prieſtley attempted to decompoſe the carbonic acid gas, by heating pieces of iron with a lens placed in it, could not poſſibly have ſucceeded, at leaſt to any ſenſible degree; for the heated air when left at liberty to aſcend, would not remain in contact with it, nor even near it, for a moment: thus every portion of air, being unconfined, would expand prodigiouſly, ſo that very little of it could come or remain in contact with the heated metal." Now, in the Doctor's experiment there was no *wet bladders*, which the fixed air was incloſed in; but he ſays, that the heated air would not remain a moment attached to the heated iron. But, Mr. Cruikſhanks, it is not being merely a chemiſt in an elaboratory. Phyſical diſquiſitions require a general knowledge. But, even as a chemiſt, you ſeem to be a very lame one; for you had no idea of water being held by chemical attraction to the earth of the chalk, as well as the fixed air. But know, Mr. Cruikſhanks, that there would be a conſtant circulation of the fixed air acting upon the heated iron; for, as one part in contact with the heated metal, it would be immediately rarified, and in conſequence fly from it; but then the cool fixed air would deſcend upon the iron, ſo that there would be a very active circulation going on, and would be *even* more conſiderable than your fixed air being propelled by two wet bladders. But, Mr. Cruikſhanks, there is another phenomenon in Dr. Prieſtley's experiment, which you are perfectly ſilent upon; viz. part of the fixed air became azote gas. This can only be anſwered *by my theory*, and directly refutes Lavoiſier's. It is clearly from the aerial acid receiving a proportion of fire, ſo as to neutralize it; that is, take its acidity from it.

[*] That this gaſeous oxyde of carbone does not require a red heat in its formation, is ſufficiently proved from the experiment of paſſing fixed

quence its fixed air and its fixed fire had been ſet looſe, and its water imbibed by the dry charcoal; but upon the heat being increaſed, this water and the charcoal form the hydro-carbonated air. Theſe metallic oxydes, as minium, in order to be formed, require to be waſhed in water; and, after it is formed, it will not give out air upon its firſt being made, till it is expoſed ſome time to the air, in order to imbibe moiſture. Mercury has a ſlow calcination, and therefore its calx imbibes its water at the time.

Need I here ſuggeſt to my candid reader the *eaſy* and *rational* explanation of *all the acknowledged phenomena of theſe experiments by both parties, become ſo eaſily explained by my hypotheſis, and how abſurd and rediculous by theirs?* Then let reaſon reſume her ſeat.

Mr. Cruikſhanks ſays (p. 44), "What, however, "diſtinguiſhes them ſtill more completely, is the large "proportion of water generated by the combuſtion of "the hydro-carbonate in oxygen gas; for the gaſeous "oxyde when pure, or burned under the ſame circum-"ſtances, never produces the leaſt ſenſible quantity of "this fluid."*

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* Alſo, the burning charcoal in oxygen gas, there is no water found: for the water that comes from the oxygen gas goes to form the aerial compoſition of the condenſed fixed air in the charcoal, The bodies which

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air and water through heated charcoal. Though very little heat is employed in the proceſs, this air is formed. Now the great heat required to produce it in Mr. Cruikſhank s experiment, muſt be owing to the immenſe heat required to ſeparate the fixed air an d water from the oxydes of iron, zinc, &c.: for when the fixed air and water is free, and paſſed through theſe calces, this gaſeous oxyde is formed with very little heat. And therefore, clearly, why the calces of mercury require leſs heat in producing of it, is evidently from thoſe calces parting with its air ſooner than other calces.

Now, Mr. Cruikſhanks, this gaſeous oxyde is formed partly of hydro-carbonated air, and alſo partly of fixed air united to charcoal, (that is water and charcoal, and fixed air and charcoal); therefore, Sir, the fixed air, inſtead of poſſeſſing water, poſſeſſes only charcoal inſtead of it. That fixed air, when aerialized, requires water for its aerial compoſition, is directly proved. That barytes, when heated by having ſteam to paſs through it, parts with its acid in an aerial form, as fixed air; but without water, no heat can expel its acid (that is by ſteam paſſing through it): part of the water goes to the aerial compoſition of the acid, or its water of compoſition, and another part unites to the earth of barytes, in place of its acid; therefore, when this gaſeous oxyde is fired with oxygen gas, the water which the oxygen poſſeſſed for its aerial compoſition is immediately abſorbed in the combuſtion, and goes to the aerial compoſition of the acid, which was united to the dry charcoal, and which is now formed into fixed air. Fixed air requires a certain degree of water for its compoſition; but when fixed air is formed into oxygen gas, it requires more water—that is, as it becomes ſaturated with fixed fire it requires more water. That there is a great quantity of fixed air formed in the combuſtion, it being double the quantity of oxygen uſed in the proceſs. Now, fixed air can only be formed in the manner I ſpeak of, which muſt be obvious to any man of common ſenſe and candour, but I find few ſuch men.

I ſhould think and expect, that, if they are open

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form charcoal are an acid and an inflammable body; ſo that, when ſteam paſſes through it, they form fixed air and hydro-carbonated air, the water giving to both of them their water of aerial compoſition.

to conviction, and have any philoſophical candour, they muſt aſſent to my explanation of the phenomena, or if they are ſo obſtinate as not to do that, at leaſt to come fairly forward and aſſign their reaſons for not doing it; but, from the uſage I have received, I know they will do neither; that neither ſcience, honour, truth, nor juſtice, will drag them forward, but ſneak away, like all the reſt, (as they did in my ſtrictures on the Galvanic Experiments), and be totally mute, depending upon the great power of the baſe combination to ſhelter their mean and ungenerous conduct. But, reader, thou muſt be ſtruck with my theory: it falls in with and explains ſatisfactorily all the phenomena, that their *own* experiments have produced. Then, what thinkeſt thou of their conduct? Do reaſon for thyſelf; and do not be led by the noſe by this baſe combination.

But only maturely weigh by the ſcale of reaſon, exempt from the vain aerial flights which chemiſtry has of late been inundated with, the different quantities of acid which Mr. Lavoiſier and the French chemiſts found in ſulphur, than which Mr. Chevenix, Mr. Lenard, and Dr. Thomſon found; the difference being ſo conſiderable as from 29 to 38 in 100: therefore, as they allow theſe gentlemen to be their firſt experimenters, ſo great a difference can only be from the different manner of their producing the acid, as I have juſt explained. But if my reader be a ſtubborn and inflexible *French votary*, let him try the experiments by both their proceſſes, and he will find directly the ſame reſults as they did.

Then, if he is not deadened to the voice of reaſon and ſcience, he muſt ſee the abſurdity of the French philoſophy, which ſays the formation of the vitriolic acid is from oxygen and ſulphur. In forming the acid by burning it in oxygen gas, it is clearly an active combuſtion, producing the moſt intenſe fire; but will he ſay that the action of the nitrous acid upon ſulphur is a ſimilar combuſtion. No; it is not a combuſtion, it is from a part of the nitric acid taking the phlogiſton of the ſulphur, and flying off with it as nitrous air, there is ſome heat produced; but that is from the acid acting upon the phlogiſton ſo rapidly, as to ſet ſome of its fire looſe as free fire. This is demonſtrated by uniting to this nitrous air ſome oxygen gas; the acid of the nitrous gas, for it is not neutralized, as it is in dephlogiſticated nitrous air: for the former turns vegetable juices red, but the latter not; and therefore will not act upon oxygen gas, producing no fermentation together: which is from this ſimple principle—the acid of this nitrous air attacks the fixed fire of the oxygen gas. But ſtill ſtronger, Dr. Prieſtley found, when theſe airs were united over water, that the nitrous acid, when precipitated, would form with the water again freſh nitrous air, the water emitting it very rapidly; it receiving, according to my theory, its water of compoſition again, and ſtill retaining the principle part of its phlogiſton, it not being decompounded by the fermentation. But the whole proceſs is fully and accurately explained (ſee my Chemical Eſſays). Reader, reflect upon theſe obſervations, and I will pledge myſelf, if thou art not an inhabitant of the French Laputa, thou muſt be convinced by old Engliſh common ſenſe and reaſon. But, to examine it according to the French theory, by ſaturating the nitrous air with oxygen gas, and then applying the produce to freſh ſulphur, we might go on to eternity with producing the ſame products. But we will find ſoon, that we can neither produce the vitriolic acid or nitrous air. I would wiſh the tyro to try it, and he will ſoon be confirmed. What, Mr. Reviewers, were you to try it? No; your province is not to try experiments, it is only to gull your humble readers—to make them even ſwallow St. Paul's, if it is neceſſary. Cloſe reaſoning and common ſenſe are unfaſhionable beings, not adapted to you, whoſe principles in chemiſtry are all *a la Francois.**

Mr. Monge is ſaid, by paſſing the electrical fire

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* I ſhould wiſh to introduce a little common ſenſe into their chemical diſquiſitions; for, till it is done, chemiſtry will be the opprobrium of ſcience. Now, every tyro knows, when a body is on fire, there is a great quantity both of fire and light comes from it. Now, attend to the combuſtion of ſulphur, even in the atmoſphere, but in oxygen gas it is moſt brilliant. Then attend to the action of the nitric acid upon ſulphur. Now, our French theoriſts ſay that theſe proceſſes are directly the ſame: indeed the latter proceſs ought to produce a more active combuſtion, as it ſooner decompounds a given quantity of ſulphur than its ignition in pure air. Fire produces fire; but the aerial chemiſts are ſo anxious to deceive themſelves in its favour, Will a combuſtible body burn, if it is placed in this mixture of the nitric acid and ſulphur? Therefore we muſt know that, in order to ſet fire to a body, we place it near a body in actual ignition — and it muſt be ſet on fire. Dr. Thomſon wants to make ſome fooliſh diſtinctions in combuſtion, in order to *mend* this abſurd French theory. But he, and every man who are open to reaſon, and know that one and two make three, know that there is far leſs heat produced in the action of the nitric acid upon ſulphur than its actual ignition in pure air. If he is a ſceptic, let him apply his noſe to them both, and he will find the difference. There are ſome who call themſelves philoſophers, who are only to be convinced by ſuch arguments as their own groſs feelings. Now, in theſe proceſſes, according to his definition, the heat ſhould be the ſame; but only there ſhould not be any light produced in the proceſs of the nitric acid and ſulphur; but the light is equivalent to the heat. They are both in a very low ſtate, producing a low temperature of heat, and a low red light. But that the nitric acid will

(after Dr. Prieſtley) through fixed air, to have produced hydrogen gas. In my laſt letter to Mr. Cavendiſh I fully proved, that is was not owing to the water of the fixed air being decompounded, but owing to the inflammable matter attached to the electrical ſpark uniting itſelf to the fixed air; and that there was no calcination of the metals employed, as their theory ſuppoſed.*

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produce actual combuſtion, is well known from the familiar facts of its ſetting fire to the eſſential oils, and when united to an alkali, and mixed with ſulphur and charcoal. In both theſe combuſtions airs are generated; ſo that Lavoiſier ſuppoſed the exploſion of gunpowder was from the airs which he found produced by it.

But there is a direct abſurdity in Dr. Thomſon's cobling of this French theory. When water is decompounded in metallic calcinations, he ſays its hydrogen receives the light of the metal, and its oxygen enters the calx. Then when the nitrous acid calcines metals, its oxygen enters the calx, and its nitrous air muſt receive the light of the metal. But then to give his cobling conſiſtency, he ſays, nitrous air is a ſupporter. Will nitrous air ſupport him by breathing it? But even admitting it, what comes of the light of the metal? for it is as perfectly calcined as when the diluted vitriolic acid is uſed, and contains not an atom of ammonia, but is the pure calx of the metal, united to the diluted nitrous acid.

* 'But ſince our *aerial philoſophers* are ſo ardently and enthuſiaſtically led 'away by the French *mania*, let us ſee if inflammable air is not ſometimes 'generated where there *is no charcoal.* M. Monge found that the electric 'ſpark would turn fixed air into inflammable air; and this, according to 'their theory, he ſuppoſes from a decompoſition of water: but, as there 'was no pure air generated, the other ſuppoſed conſtituent of water, he 'ſuppoſes it muſt have combined with the mercury. But Mr. Monge un'fortunately had not ſuch a friend as Mr. Rupp, with his keen viſion, to 'diſcover that the mercury was calcined in the operation, which we know 'it muſt have been if the mercury had imbibed it. Yet, true it is, the ſame 'phenomena will take place if the fixed air is electrified over water.*

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* 'Beſides, as the electric fire only paſſed from one conductor to the 'other, and only paſſing through the fixed air would not act upon the 'mercury: alſo, how could they ſuppoſe, if pure air had been ſet looſe 'in the proceſs, it would act on the mercury? But all theſe reflections 'are foreign to our aerial philoſophers; their reaſonings, according to 'a perſon in the Gentleman's Magazine, "it muſt and does."

Now, M. De Saſſure, jun. has been performing ſome ſimilar experiments; and he clearly proves, after me, that it is not owing to the decompoſition of water: for he ſays in the experiments, that the metals "were not "ſenſibly calcined"— that is, not oxidated: and he alſo ſays, that the fixed air is in part changed into the gaſeous oxide of carbon. Now, how beautifully this agrees with my theory, that the gaſeous oxyde is formed of fixed air and inflammable matter. My experiments received no attention; but ſince they are confirmed by M. De Saſſure, I hope they now will. No! though theſe experiments are puffed, as M. Saſſure brought them in favour of Mr. Cruikſhank's opinions; but ſince his opinions are abſurd, they will be dropt into oblivion. He ſuppoſes that the electrical fire takes oxygen from the fixed air, —then what became of it—the metallic wires are "not ſenſibly calcined," (I have found golden wires not in the leaſt)—and it totally diſappears. That the electric ſpark contains inflammable matter I have long ſhewn, even before Volta's electrical experiments. By the reader referring to M. De Saſſure's experiments, he will diſtinctly ſee (as it is clearly proved by them) that the phenomena are not from the decompoſition of water; and alſo, that what they call the gaſeous oxyde of carbone is formed, and not hydrogen gas.

'Now, Sir, attend to my explanation. This electric fire will give the 'calces of metals ſuch a quantity of fire as to reduce them; if paſſed 'through the phoſphoric acid, will, from the ſame cauſe, form it into in'flammable air (and alſo common fire will do the ſame with the phoſphoric 'and the calx of lead), and, if paſſed through the nitrous acid, will give it 'ſuch a quantity of fire as to form it into an air that will allow bodies to 'burn in it with a moſt brilliant and enlarged flame: alſo, if paſſed 'through the cauſtic volatile alkali, will give it ſuch a quantity of fire as 'to form it into inflammable air. Likewiſe fixed air is formed into in-

Mr. Lavoiſier, who is ſo puffed for the accuracy of his experiments in forming his theory, but upon the repetition of theſe experiments, moſt of them have been found groſsly erroneous; upon burning oxygen gas and carbone, ſays, that the fixed air produced is equal in weight to the oxygen gas and charcoal. Now, Mr. Cruikſhanks found that the fixed air was not equal in weight.

Now, upon oxygen gas being burned with charcoal, the fixed fire of the oxygen gas, and alſo of the charcoal, are ſet looſe; and the fixed air of the oxygen gas, and alſo of the charcoal, remain: but they do not amount to the weight of both theſe bodies, the water of the oxygen going to the formation of the charcoal's fixed air. But there are ſome extraneous bodies in the charcoal, otherwiſe the fixed air ought to be more; as, by the oxygen gas and charcoal loſing their fixed fire, they ought to become heavier, as I have clearly proved that fire is a principle of levity, from repulſion.

Dr. Girtanner ſays, (ſee Nicholſon's Journal for September, 1800, p. 268) "Another experiment, which "appears to me to confirm my opinion, was made by Mr. "Henry, of Mancheſter. He repeated it ſeveral times, "and always with the ſame ſucceſs. His explanation "of this phenomenon may be ſeen in Scherer's Journal, "which is different from mine, becauſe he conſidered

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'flammable air by fire; for, if pure limeſtone is expoſed to a ſtrong heat, 'it will diſcharge inflammable air, owing to calcareous earth having ſo 'ſtrong an attraction for its air, that it will not part with it all till a part 'has got ſo ſtrong an impregnation of fire as to form inflammable air †.

'† It cannot be from decompounding water, for here is no metal 'to attract the other component part, pure air.'

" azote as a ſimple body. His experiment is as follows: " In a recurved tube over mercury a mixture was made " of 94.5 meaſures of carbonated hydrogen gas from " acetite of pot-aſh, and 167.5 meaſures of very " pure oxygen gas obtained from the oxygenated muri- " ate of pot-aſh. The mixture amounting to 202 mea- " ſures, was reduced, by an electric exploſion paſſed

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'But if fixed air is united to the earth of mercury, as when a ſolution ' of mercury in the acids is precipitated by the fixed alkali, the acid unites ' to the alkaline ſalt, and the fixed air to the mercury.' — See Doctor Prieſtley, vol. vi. ſect. vii. p. 162.

' This calx of mercury, by heat, will part with its fixed air as pure air; ' and that there is a near connection between pure and inflammable air ' is clear; for Doctor Prieſtley, in the ſame proceſs, ſometimes formed his ' nitrous dephlogiſticated air (as he calls it) into an air that would admit ' of bodies burning in it with an enlarged flame, and ſometimes ſo en- ' larged as to burn all at once, the ſame as inflammable air.

' Now if this air is attended to, it will clearly expound the whole myſ- ' tery. If the acid vapour is ſlightly mixed with phlogiſton, or has got a ' ſlight impregnation, it will only form nitrous air; but if this nitrous air ' is expoſed to iron it will corrode the iron, taking its phlogiſton from it, ' ſo as to form the air that will allow a candle to burn in it with an en- ' larged flame. But ſometimes, in the ſame proceſs, (ſee Dr. Prieſtley) it ' got ſuch a conſiderable proportion of phlogiſton, or fixed fire, as to ex- ' plode all at once. And that theſe phenomena, according to my theory, ' are clear; for, attending to the different impregnations, it will burn dif- ' ferently; ſometimes the flame of the candle will be ſmaller, ſometimes ' larger; that is, ſuppoſe a candle whoſe flame in burning in atmoſphe- ' rical air, ſhould be the ſize of half an inch; in the nitrous air (we will ' ſuppoſe in ten inches of this air) its flame will ſometimes extend to one ' inch of the air, ſometimes to two inches of the air, and ſo on, and ſome- ' times it will extend to the whole ten inches, it burning the air all at ' once. And that it is not the ſame inflammable air which comes from ' metals, is eaſily aſcertained by putting it to the different teſts; but it is ' preciſely an air formed of the nitrous acid, with a ſtrong impregnation ' of phlogiſton or fixed fire.' See my ' Thoughts on Air.'

"through it, to 128.5 meaſures, and afterwards by lime "water to 54.0 meaſures. So that in this experiment "23 meaſures of azote gas were produced by the electric "exploſion of the oxygen united with the hydrogen."

Now, clearly, a great quantity of azote gas was formed in this experiment; and it can only be explained by the combuſtion being ſo imperfectly conducted, as not to burn all the oxygen gas into fixed air, but a great part of it was burned into azote gas. And this controverſy, like all the reſt, (for I make this general declaration, that performing experiments different ways, the phenomena, according to their theories, will be different) is carried on by contradicting the clear facts of each other. But if the reader will examine their facts by my theory, he will clearly account for all of them.

And alſo the burning the oxygen gas and hydrogen gas by the electric ſpark, that they burn with a ſlow combuſtion. Then I only aſk this ſimple queſtion: how comes it that by the ſame combuſtion, conducted in this manner with oxygen and hydrogen gaſſes, that azote gas is burned (as they ſay it is) when the nitrous acid is produced? But every one knows and allows that, when the oxygen gas and hydro-carbonated gas burn without the aid of the electric ſpark, that they only form fixed air; but by the electric ſpark they form a great quantity of azote gas, it appears. It is juſt the ſame in that ſuppoſed famous experiment of the French, in producing water only from oxygen and hydrogen gaſſes. Dr. Prieſtley, when ſpeaking of it, ſays, (ſee his Doctrine of Phlogiſton eſtabliſhed, p. 44 *.) To carry the analogy

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* "Another pretended proof that water is compoſed of dephlogiſticated and inflammable air, is that when the latter is burned ſlowly in the for-

on: by the combuſtion of that pure inflammable air from the metals, they burn the oxygeous acid into the mineral acids; but in the combuſtion of an impure inflammable air from charcoal, not poſſeſſing ſo much fire, they leave the oxygeous acid in an aerial form, as fixed air or the aerial acid.

Now, in this experiment of the French (which they ſay was ſo deciſive of their doctrine) a great quantity of azote gas made its appearance, which was clearly formed in the ſame manner as in Mr. Henry's experiment, by the very imperfect combuſtion. Their experiments were performed by firing the gaſſes by the electric ſpark, and that in a very ſlow and gradual manner: and, as Dr. Van

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mer, they both diſappear, and a quantity of water is produced, equal to their weight. I do not, however, find that it was in more than a ſingle experiment that the water ſo produced is ſaid to have been entirely free from acidity, though this experiment was on a large ſcale, not leſs than twelve ounces of water being procured. But the apparatus employed does not appear to me to admit of ſo much accuracy as the concluſion requires; and there is too much of correction, allowance, and computation in deducing the reſult.

"Alſo it is, after all, acknowledged that after decompoſing this quantity of the two kinds of air, and making all the allowance they could for phlogiſticated air, or *azote*, in the dephlogiſticated air, they found fifty-one cubic inches of this kind of air more than they could well account for. This quantity, therefore, and perhaps ſomething more (ſince the operators were intereſted to make it as ſmall as poſſible) muſt have been formed in the proceſs. They acknowledge that, except when the inflammable air was burned *in the ſloweſt manner*, the water they produced had more or leſs of acidity."—— *So far Dr. Prieſtley.*

As to their aſſertion that the water remaining is equal in weight to the airs burned, all other experimenters found the water is leſs in weight. But why the water is fouud ſo heavy, is owing to the airs poſſeſſing ſo much fire, and this fire counteracting gravitation.——(See my ſyſtem of Fire.)—Therefore the water, when bereft of the fire, would become heavier than it was before in its aerial ſtate.

Marum obſerved, if a foot of inflammable air is fired in leſs time than eight hours, he always found an acid in the reſiduum. Then, in the little quantity of oxygen gas that would be neceſſary to fire the hydrogen gas, and as a great reſiduum of azote gas is always found in it, when fired ſlowly, is not the mode of its being produced clear and evident. Beſides, Mr. Cavendiſh, if you are open to conviction and common ſenſe, I ſhall aſk you and all your votaries this obvious and ſimple queſtion: as there is a great reſiduum of azote gas in theſe experiments, would it not, according to your opinion, have formed with the oxygen gas the nitric acid, as you ſay theſe two gaſſes form it by the electric ſpark? Now here it appears, that, in theſe experiments, inſtead of the electric ſpark (which is for eight hours conſtantly acting upon them) forcing them into the nitric acid they are by it actually formed into azote gas, juſt the reverſe of what your *famous* experiment taught. But as it appeared in that experiment, the cauſtic alkali decompounded the air (ſee my experiments and obſervations on the electric pile, p. 20); and alſo, that I have clearly proved beyond all doubt (and I challenge the combination upon it) that the electric ſpark will attract the acids, and fly off with them, they even penetrating the metals; and alſo in Mr. Cruikſhanks's Experiments upon the Pile, and alſo Dr. Wollaſton's, the electric ſpark contains an acid, even throwing mine out of the queſtion: then, can you any longer place any confidence in your abſurd opinion, that the nitric acid is formed of oxygen and azote?

Their theory appears extremely abſurd, in accounting for the airs that are produced from the nitrous acid, ſo that their own modeſty ſeems to acknowledge the difficulty and inconſiſtency. The nitrous ammonia may be almoſt all formed into what they call gaſeous oxyde of azote, only a little fixed air in it, as Dr. Auſtin clearly proved. This air favours combuſtion more rapidly and intenſely than even oxygen gas; but it kills animals in three or four minutes, as Mr. Davies allows: *yet Dr. Beddoes and he, with other gentlemen of Swift's iſland, breathed it with moſt ecſtatic ſenſations.* But I think now they are aſhamed of their abſurdities, ſo as to wiſh to bury them in oblivion. But theſe facts are clearly deduced: that it is as noxious to animal reſpiration (the inhabitants of Laputa excepted) as any air; that it allows bodies to burn in it with more brilliancy than even oxygen gas; and that the nitrous ammonia may be all formed into it. Then what does it conſiſt of? Why, of the volatile alkali; and alſo a volatile acid. By heat the acid and alkali are volatilized, forming this air. No, ſays our French theoriſts; the acid and alkali are decompounded. Then they conſiſt of azote, oxygen, and hydrogen, according to them, though we are obliged, by ſwallowing the elephant, to ſuppoſe that theſe very *volatile* bodies, in the act of evaporation, are decompounded; yet, if that was the caſe, we ſhould have expected to have found their conſtituents in the air; viz. azote, oxygen, and hydrogen gaſſes: or, as they decompound and compound at pleaſure (if both take place in the proceſs), we ſhould have expected to have found water, the nitrous acid, and azote. But we find a more active air for combuſtion than oxygen gas, with ſome fixed air; yet it is not reſpirable to *common mortals.* From whence comes it? The azote and hydrogen gaſſes, when mixed with oxygen gas in theſe proportions, do not hinder animals from breathing it. But the oxygen muſt be purer in this air than in pure oxygen gas, as animal bodies burn in it with greater brilliancy; and alſo it is conſiderably lighter than oxygen gas. Still the difficulty increaſes, as there either muſt be no decompoſition of the acid and alkali; or there muſt have been, after the decompoſition, a compoſition again, as there is no inflammable air in it, as bodies burn in it without the airs exploding, which muſt have been the caſe. Reader, if thou art ſtill an inhabitant of this iſland, and not of Laputa, thy common ſenſe muſt be gravelled—nothing can extricate thee. Thou muſt apply to the *wonderful and learned* combination, and their very *modeſt, candid* agents, the Reviewers and Journaliſts. They will do it at once, by a ſignificant ſhrug of their ſhoulders, and only gape wide, and the elephant is whiſked down thy throat immediately. But if thou art a reader who art determined to think for thyſelf, I ſhall reaſon a little more with thee.

Nitrous air, if applied to needles, calcine the needles, and forms this gaſeous oxyde of azote: how comes it this air ſhould have been formed? Reaſon ſhould have expected, that the oxygen which the nitrous air poſſeſſed, ſhould have gone to the oxyding the iron; but here is an oxydation, and at the ſame time a generation of oxygen gas: ſwallowing the elephant here will not ſerve thee, it muſt be a whale. And if the whale is ſwallowed, after ſuch an operation, whoever thou art, thou haſt undergone thy probation—thou art now qualified to be an inhabitant of Laputa, and I have done with thee; go to bed, and, like the huge ſerpents which we read of, who ſwallow buffaloes; ſnore away thy time till the whale is digeſted, for, while it is on thy ſtomach, thou art not a reaſonable being. But I muſt here invoke reaſon and common ſenſe to accompany me. When the nitrous acid is applied to iron (as this acid has the ſtrongeſt attraction for phlogiſton—an old fact, well eſtabliſhed by our ſolid forefathers) it will attract the phlogiſton from the metallic earth, and form nitrous air: but the acid ſtill predominates in it, as it will turn the vegetable juices red, as Dr. Prieſtley found; but if applied to more iron, as he alſo found, this gaſeous oxyde of azote would be formed, and it would loſe its power of turning vegetable juices red, its acid being now *more perfectly ſaturated* with phlogiſton. Alſo another eſſential fact: the nitrous gas attacks the fixed fire of oxygen gas, and the chemical action induced, they decompound each other. But, as Dr. Prieſtley found this gaſeous oxyde of azote would not act upon oxygen gas; no, the nitrous gas acts by its acid having a ſtronger action upon the fixed fire of the oxygen gas than its own acid has, that being only now fixed air, or the aerial acid. We need not be ſurpriſed at this, ſince we ſee, through all chemiſtry, that the ſtrong acids always expel the weaker, an old fact of our ſolid forefathers. But all theſe ſolid facts are nothing to the *aerial facts* of our modern chemiſts. They had not the art of ſwallowing whales. The ſtrong nitrous acid, when applied to ſtrong phlogiſtic bodies, as the volatile oils, will ſo attract their fixed fire, as to ſet it looſe as free fire, producing combuſtion. Then, need we be ſurprized at this gaſeous oxyde of azote burning: nay, Dr. Prieſtley formed it of ſuch a high concentration of fixed fire, when heat was applied to it, that it burned and inflamed of itſelf.—(See my Letter to Sir J. Banks, in the Gentleman's Magazine for December, 1794; or in my laſt Letter to Mr. Cavendiſh, page 19, and there the whole is fully explained). Nay attend, you *aereal flying* chemiſts, to the name our forefathers gave the nitrous ammoniac; —they called it *nitrum flammans*. Then, need we be ſurpriſed at its inflaming when formed into air, when it would in its ſolid ſtate? O you conjurers!—But the French chemiſts, in their experiments upon ammonia, as they could produce water (though it was the water of compoſition), attributed it to a different cauſe; but they could not produce azote, which, according to their theory, ought to have been: but, puff! that is nothing, no obſtacle at all—only ſwallowing a mite.

I ſhould wiſh our aerial chemiſts particularly to attend to this gaſeous oxyde of azote, its weight, the materials it is made from, and I think it is almoſt impoſſible to imagine it is formed as their theory ſuppoſes, the nitrous flammans being juſt aerialized by a gentle heat: then, how could the ammonia and acid be decompounded; it is truly ledgerdemain work. It is as extraordinary to me, as to ſuppoſe that our Reviewers are men of the greateſt ſenſe, learning, integrity, candour, and honour*.

(In the Philoſophical Magazine for February, 1803, p. 93.) "5. We are informed by Mr. Paul of Geneva, "(now in London) that water holding ſulphate of lime "in ſolution, and impregnated under preſſure with hy-"drogen gas, of which it can take up about one-fourth "of its volume, after ſtanding ſtopped up for about "three months, on being opened was found to contain

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[*] But need we be ſurpriſed at our aerial chemiſts ſuppoſing that the nitrous ammoniac being decompounded by being evaporated by heat, when they ſeriouſly advance that water is decompounded in the act of evaporation? To ſuch groſs abſurdities I ſhall not deign to reply.——Need we be ſurpriſed at the acid of nitrous air decompounding oxygen gas, when Dr. Prieſtley found that the pure nitrous vapour would even decompound nitrous air?—See vol. ii. p. 171.

"ſulphurized hydrogen gas. This has been repeated "ſeveral times with the ſame reſult. In diſtilled water "not impregnated with ſulphate of lime, the hydrogen "gas undergoes no change.

"How is the ſulphurized hydrogen gas produced? "There is plainly a decompoſition effected, not merely "of the ſulphate of lime, but of the ſulphuric acid, which "parts with a portion of its ſulphur.—What becomes of "the oxygen? Does it join the hydrogen, and form "water?

"The hydrogen gas made uſe of in the above experiments was procured from the decompoſition of water, "by applying it to iron expoſed to a high temperature. "When obtained by means of ſulphuric acid, water, and "iron, in a humid way, it has a different flavour, ſomewhat hepatic."

Now we ſee how their experiments embarras them: they are obliged to compound and decompound at ſuch a hocus pocus rate, it is truly ridiculous, *that they themſelves ſeem now to be ſtaggered.* The explanation is ſimply this: — the vitriolic acid having a greater attraction for the hydrogen gas than for the lime, unites with it, forming ſulphurized hydrogen gas. Sulphur is only the vitriolic acid united to phlogiſton, as the old chemiſts thought*. I alſo ſee, from the Critical Review, that Mr.

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* Let us ſee how, what they call ſulphurated hydrogen gas is formed. Sulphur united with the cauſtic alkali, upon water, and the ſulphuric acid being added to it, they penetrate the alkali, ſetting the ſulphur and the fire of the alkali free, and the ſulphur, along with the water, aided by the fire are aerialized. If the water was decompounded, we ſhould have formed in the proceſs the vitriolic acid air; but there is no more vitriolic acid found in the proceſs than the vitriolic acid which was added to the mixture. There is ſome fire ſet looſe from the ſulphur, but not enough to

Smithſon has proved that calamine contains water and carbonic acid. It ſays, "Water in the calx of zinc "ſeems to be in a ſtate of peculiarly intimate compo- "ſition, and it is called, by our author, 'hydrat of zinc;' "and the whole of the ingredients of this ore appear to "be in a ſtate of chemical combination." If Mr. Smithſon had attended to my writings, I ſhew that all the native ores of metals are compoſed of the metallic earths, and principally with water; but ſometimes with the different mineral acids and water; and ſometimes fixed air and water; and ſometimes ſulphur; and ſometimes earths and water, and even other bodies; but not any oxygen gas — that conjurer, that does ſuch wonders in the creation.

I have often been ſurprized at all our old chemiſts running with ſuch headlong impetuoſity to this abſurd French theory; but I belive the truth was in ſome meaſure to fly from mine. Let us only conſider that obvious experiment of the liver of ſulphur attracting oxygen gas, ſo that it is uſed as an eudiometer. Let us maturely conſider this alkaline ſulphuret, which is formed of an alkaline ſalt, united to a great quantity of fire and ſulphur. Upon water and oxygen gas acting upon it, the water penetrates the alkali, and ſets free its fire, which

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produce it into an acid. Alſo it is obſerved, that the hydrogen gas produced from metals by the diluted vitriolic acid always has an hepatic ſmell, which clearly proves that a little of the vitriolic acid (united to the phlogiſton of the metals) is taken up along with the water in forming this air. We always allow that the ſmell of the nitrous acid indicates its preſence; why not the vitriolic? By adding water to an alkaline ſulphuret, the water, aided with the fire of the alkali, will in ſome meaſure decompound the ſulphur of its fire, forming a kind of an acid; for this reaſon Berthollet found it in ſome proceſſes act the part of an acid.

then, along with the oxygen gas, ſets fire to the ſulphur. But as this burning of the ſulphur is immerſed in water, in conſequence it is not active, but a ſlow combuſtion; and a part of the ſulphur is precipitated as ſulphur, and alſo the oxygen gas of the atmoſpherical air is not all imbibed by the ſulphur, but a great part forms with the cauſtic alkali, the mild or carbonated alkali, from this ſlow combuſtion. Dr. Gren ſays*, (vol. i. p 250.)

He alſo gives this explanation of the phenomena, according to the French theory†.

But, entirely to deſtroy this abſurd ſuppoſition of the decompoſition of water, let us uſe a little *common ſenſe.* It is only in water that this alkaline ſulphuret ſoon undergoes a total decompoſition; for if it be expoſed to the dry air, it is a long time: therefore we have a right to ſuppoſe, according to them, that the water is decompounded. But it is ſingular, that the alkaline ſulphuret ſhould decompound the water, when it had the oxygen gas to act upon. Beſides, what became of the hydrogen? — But as they compound and decompound water at pleaſure, under all attractions and temperatures, they will ſay it unites to the oxygen gas, and forms water. Here then is a little extraordinary conjuration, (or hocus pocus work), that this fickle, capricious conjurer, *Hydrogen*, ſhould be torn from its dear *Oxygen*, in order to form a new connection with ſome freſh *Oxygen**. O you unconſtant paramour!—But, to deſtroy all this gallantry; from whence comes the carbonic acid, to form the carbonated alkali? They ſay, from the atmoſphere. But, to deſtroy all theſe *abſurd*, *fooliſh*, and *ridiculous* high-flown Gallican flights, waſh the pure air which the dry alkaline ſulphuret is to act upon, in lime-water previous to the experiment, and nevertheleſs the carbonated alkali is found. I ſhould wiſh the tyro to perform this ſimple experiment: alſo let him take a common pint bottle, and place a little lime-water in it; then ſhake the water well in the bottle, ſo as to attract this ſuppoſed fixed air that is floating looſe in the atmoſpheric air, or oxygen gas, if it is filled with it; or let the air in the bottle paſs through lime-water. After that, place the mouth of the bottle (with this waſhed atmoſpheric air or pure oxygen gas in it) in a quantity of ſulphuret diſſolved in water; then cork the bottle, and he will ſoon find all the oxygen gas imbibed by the alkaline ſulphuret; and nevertheleſs, the alkali will be carbonated, and in quantity bearing a conſiderable proportion to the oxygen gas loſt in the experiment. Now I muſt exhort my readers to make this experiment; and if they poſſeſs common ſenſe and candour, I will pledge myſelf they will be convinced. Here then is the carbonic acid formed without charcoal. — I could mention many other deciſive experiments: but if they will not be convinced with what I have already ſaid, I give them up to the French *mania*, as unconquerable fools. (This may be thought harſh language; but they certainly deſerve it, on account of their unparalleled behaviour to me.

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* "Alkaline ſulphuret, diſſolved in water, undergoes, when expoſed to the atmoſphere, a total decompoſition. The ſolution, which was clear before, grows turbid; ſulphur is precipitated, and the remaining lye contains at laſt nothing elſe but ſulphated alkali, mingled with more or leſs carbonated alkali. Dry alkaline ſulphuret likewiſe decays in open air, loſing all its peculiar properties ſo far, that, finally, nothing elſe remains but ſulphur and ſulphated alkali, together with carbonated alkali in different proportions."

† "The ground of the reciprocal actions of liver of ſulphur and vital air upon each other, lies in the attraction which both ſulphur and hydrogen have for oxygen; and by this it is eaſy to explain why the alkaline ſulphuret is after ſome time converted into a ſulphat.—Another cauſe of the deſtruction of the ſulphuret of alkali in common air, is to be ſought for in the carbonic acid. The alkali attracts this acid, met with in the atmoſphere; is by it transformed into a carbonat; and hence rendered incapable of holding ſulphur in ſolution, which of courſe muſt precipitate."

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* And this oxygen to be in its aerial ſtate, or united to caloric. The reaſon they aſſign why hydrogen gas and oxygen gas do not unite, is owing to their caloric. Yet here the oxygen quits its hydrogen, in order to unite with freſh oxygen, with its caloric as oxygen gas.——But they blunder from one abſurdity to another, and even contradict themſelves!

Whatever way we look at the theory, the moſt groſs abſurdities ſtare us in the face. Dr. Gren ſays, "White "lead, when acted upon by red heat, gives out carbonic "acid gas, and becomes glaſs, or vitrious oxyd of lead. "But when roaſted on a more gentle fire, it may eaſily "be converted into red lead."——This white lead is formed by the acetic acid: — here it evidently appears, that this acid forms both fixed air and alſo oxygen gas, with the lead. Now let us ſee what airs it forms with other bodies. Dr. Prieſtley, with one ounce of this acid, mixed with two ounces of calcined whiting, got from it by heat, in an earthen retort, upwards of ſix hundred meaſures of air, formed principally of azote, but both fixed air and oxygen gas mixed with it; and might have gone on till all the earth was conſumed in producing theſe airs by freſh acid.—(See Doctor Prieſtley, vol. iii. p. 202.)—But ſtill more extraordinary and more directly repugnant to their hypotheſis, is that, when this acid is united to pot-aſh, it will then (as Mr. Lavoiſier ſays) "give out ammonia in diſtillation*." Now to argue upon theſe facts, according to their ſyſtem, is too ridiculous; but I will ſhew how it agrees with mine.—All airs are formed from the acid being aerialized with fire and water, and, according to the acetic acids attraction to theſe bodies, it will be eaſily ſeparated from them.—To the earths it has the leaſt attraction; therefore with the calcined whiting, it formed an air of a leſs ſaturation of fire, (as Dr. Prieſtley ſays) "of which about one-third was fixed air, but more of this in the firſt portions, and leſs in the laſt. The ſtandard of the reſiduum, in the firſt portions, was 1.66—in the ſecond 1.42—and in the third 1.38, which was very near the purity of common air."——But when it is united with lead, it forms air of greater purity, if roaſted "on a gentle fire." But I need not inform my reader, that acids have the ſtrongeſt attraction for alkalies; and, as Mr. Lavoiſier ſays, it gives out ammonia in diſtillation;—therefore it requires along with the alkali, ſuch a ſtrong ſaturation of fire, as actually they form an inflammable air as ammonia. It is juſt the ſame with the phoſphoric acid; in paſſing the electric ſpark through it, they form an inflammable air, and if united to the calx of lead, the ſame air, as Doctor Prieſtley found: for he ſays, "The air was ſtrongly inflammable, and yielded it very plentifully."

I think I have ſhewn the abſurdity of their ſyſtem, and the truth of mine, that no one can miſtake it,—well knowing that the combination is ſo powerful, with their

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[*] That pot aſh is formed of fire, but not of ſuch a high ſaturation of fire as the volatile alkali; but it will of itſelf perform the office of phlogiſton, in reducing the calx of ſilver. Wenzel's method: "The oxyd is triturated with equal parts of dried and hot pot-aſh."

malignant arts, that without it was as glaring as the noon day, it would not be received. But even the *ſervum pecus*, I hope, muſt now be convinced, at leaſt ſo far as to demand a fair and candid diſcuſſion. But thoſe men who ſee and know the force of my arguments, what will future ages ſay to them, who had neither the ſpirit, integrity, nor candour, to come fairly forward? But let this baſe combination tyrannize, and oppreſs Truth and Juſtice; I ſhould think they would not receive their bleſſing, but it will be an eternal ſtain upon the preſent age. I cannot charge the foreign chemiſts; for this combination laid their plots ſo well, that they hindered it from being known to moſt of them, I believe*.

Nay, what cannot our modern aerial philoſophical chemiſts perform? — Turn charcoal into diamonds, as they ſay they have already turned diamonds into charcoal; iron pokers and iron tongs, into gold; water into good Britiſh ſpirits; nay, they have actually ſet fire to the Thames, that hitherto opprobrium of the ſagacity of the cocknies. And the gentleman who firſt performed this wonderful fete, ſays, he made it actually explode, or thunder, in its combuſtion (ſee the Philoſophical Tranſactions)—Calling out thunder from the waters of the deep. Well done, little Jove! I think the citizens ought to erect a monument, and place it near their other monument of fire — the one recording where fire ceaſed, and the other where it began.

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* Then, according to the Dutch chemiſts, by paſſing azote through hot charcoal, that even this air attracts a part of the charcoal, and becomes inflammable: then, can we be ſurpriſed if fixed air receives a part of the charcoal, and becomes inflammable. The French chemiſts contradict this experiment; but then they employed charcoal ſo burnt, that it had loſt its inflammable matter. The experiments

They acknowledge themſelves that they cannot obtain any hydrogen gas, but from inflammable bodies: I think

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by Clement and Deſormes (ſee the Philoſophical Magazine for July, 1803, p. 163), " Theſe chemiſts, by making azote paſs upon the char- " coal inſtead of carbonic acid, rendered it inflammable. We long " ago made this experiment with charcoal well burnt, and the azote " acquired no inflammability.

" The ſame chemiſts, to prove their aſſertion, ſtate, that copper " does not decompoſe the carbonic acid, as iron does in the experi- " ment of Cruikſhank. But it may be ſaid in anſwer, that as the " former metal does not decompoſe the water, it may not have the " property of decompoſing carbonic acid.

" In a word, the combination of inflammable gas with ſulphur, ob- " tained by the Dutch chemiſts, and which they took to be ſulphu- " rated hydrogen, was only a mixture of that gas with the gaſeous " oxyde of carbon a little ſulphurated, of which we have already " ſpoken.

" Carburated ſulphur is not a diſcovery entirely new: ſince our " labour we have learned that it had been announced in ſome work " or other before."

Alſo it clearly appears that this gaſeous oxyde is formed from water and fixed air, as they could not produce it by copper. Now, it is well known that ſteam will not calcine copper, but only iron; therefore, it muſt convince every unprejudiced mind that it is from the action of the water that the iron is calcined, that is bereft of its fixed fire, as fixed air will not. The French chemiſts are right in ſaying, that the inflammable gas with ſulphur was only a mixture of that gas with the gaſeous oxide of carbon: its weight will ſufficiently aſcertain that. Nay, Mr. Tilloch ſays, (Philoſophical Magazine for February, 1803), " A new gas, ſulphurated azotic gas, has been diſco- " vered in the mineral waters of Aix-la-Chapelle by M. C. Gimbernat, " ſub-director of the muſeum of natural hiſtory at Madrid, who gives " the following account of it:

" I have juſt finiſhed my labour on the analyſis of the ſulphureous " waters of Aix-la-Chapelle. I have found that the excipient of the " ſulphur in theſe waters is not hydrogen, as has hitherto been ſup- " poſed and aſſerted, but azote. Theſe waters contain no ſulphurated " hydrogen.

this a ſtrong preſumptive proof that its inflammability comes from the inflammable body, and that the water

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“ Sulphurated azotic gas is a chemical body, the exiſtence of which “ has never been ſuſpected. Nature preſents it in abundance in the “ mineral waters of this country.

“ Beſides ſulphurated azote, theſe waters contain a great deal of “ pure and free azote interpoſed, and in the ſemi-gaſeous ſtate.” Now, need we be ſurpriſed at this air taking up ſulphur, when even azote will do it: therefore, in many airs where ſulphur or phoſphorus is aerialized, it is not by hydrogen gas — (See ſome experiments made by Mr. Accum) where water aerialized them both; and he ſays, from his prejudiced opinions, that it was from the water being decompounded. Even charcoal will aerialize ſulphur. I ſhould only aſk our French chemiſts, if they can form this gaſeous oxyde by paſſing dry fixed air through iron. But, as Mr. Cruikſhanks and every one open to *common ſenſe* muſt know that this air is formed (when paſſing fixed air through charcoal) by the fixed air receiving a part of the charcoal, as the French chemiſts formed it of from 45 to 52 parts in 100 of carbone; therefore it could not poſſibly be ſuppoſed to receive this quantity of carbone from the metals. But could azote, you conjurers, act otherwiſe upon the charcoal than imbibing a part of it? It has no oxygen to loſe. It is really hard to have ſuch groſs abſurdities to combat. But ſuch has been my fate with this illiberal and uncandid age. I might ſay —— but I will not, reader, if thou poſſeſs honour, reaſon, and common ſenſe, thou may make out the reſt*.

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* In an extract from Mr. Sennebier's ſupplement, I ſee he has made ſome experiments which clearly ſhew what I have long proved, that vegetation turns pure air to fixed air: and alſo he found, after me, that atmoſpheric air was far more favourable to vegetation than oxygen gas;— but not owing (as he ſuppoſed) from the azote it contains; no it is owing to its being an air more eaſily decompounded of its fixed fire, as my works have clearly ſhewn. Take azote, and mix it intimately with the oxygen gas of the elaboratory, in the ſame proportion as the atmoſphere, and it will not by any means produce vegetation in the ſame vigorous manner as the atmoſphere. I ſee that Mr. Sennnebier alſo found that mixing oxygen gas with hydrogen gas, upon the former being formed into the aerial acid, the acid united to the hydrogen gas; and they produced (I found) the gaſeous oxide of carbone. That oxygen gas is not even favourable to reſpiration, whoſe organs are more able to act upon it

only acts by giving it its water of compoſition: for all airs require water before they are formed. In order to expel the fixed air from bodies, the acids to expel it muſt

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Gentlemen of *this honourable combination*, I will relate to you a true ſtory. A boy, eleven years of age, happening to caſt his eye upon ſome (what he called the *funny*) parts of my writings, exclaimed, "This is realizing "the fable," which he told in the following manner, I only varying it a little, altering his language.

"Some *profound* philoſophers peeping thro' a teleſcope, anxiouſly exa-"mining the ſun, as it had at that time become the faſhion, one of them "was ſurpriſed to ſee an immenſe animal make his appearance, covering "nearly the whole body of the ſun, with immenſe legs, and a more "immenſe proboſcis. This being diſcovered, without maturely reflecting "upon it, he immediately publiſhed it to all his neighbours, as an extra-"ordinary phenomenon, and they all immediately ſet about laying their "wiſe noddles together, to inveſtigate the cauſe and its conſequences, de-"ducing from it the moſt wonderful and aſtoniſhing theories, ſo as to "make all the town and country, and the public journals, gape with "wonder; and the bellman went about bawling, 'Surpriſing diſcoveries. "—Wonderful!—Wonderful!—Wonderful! This animal they ſuppoſed "to be able to deſtroy the whole planetary ſyſtem, by eating up the ſun," equally as deſtructive to us as the firing of the ocean might be. For, as we, *little ſtrutting animals* in the creation, had already ſet fire to water, it was eaſily extending the combuſtion to the ocean, which general conflagration muſt have extended to the earth: ſo we ſee that Mr. Cavendiſh and Dr. Pearſon's *diſcoveries and theories* might be equally as fatal to our earth as this equally wonderful long-legged, huge animal. "At laſt a "man, not having his head formed for theſe marvellous wonders, deſired "to ſee the inſtrument; and, upon cloſe examination, found a fly had in-"troduced itſelf upon one of the glaſſes. Lo, the wonder ceaſed!"—— The water depoſited in firing hydrogen and oxygen gaſſes, which you found, Mr. Cavendiſh, has had a ſimilar conſequence as the fly.

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than thoſe of vegetables, I have clearly ſhewn. Dr. Prieſtley ſays. (vol. v. p. 161.) "I found that mice would not live in dephlogiſticated air, [oxygen gas], till they had completely phlogiſticated it; though they lived longer in it than, in proportion to its purity, with reſpect to common air; and for this I cannot aſſign any ſufficient reaſon."——Now here it clearly ſhews, that the mice's blood was not able to injure all the oxygen gas; and what it did act upon, it operated ſo ſlowly, that it injured the air leſs in proportion. Vegetables imbibe hydrogen gas, which gives them a rich green colour. It is owing to this air, ſo highly formed of fixed fire that vegetables which grow in coal mines have been found ſo green.

be mixed with water. In barytes, no fixed air can be got from it by heat alone, it requiring the ſteam of water, which is ſtrikingly the caſe in metallic calces; minium, by the heat required to make it, does not poſſeſs water enough for its oxygen becoming aerial. Therefore it requires to be expoſed to the air for ſome time, in order to attract the moiſture neceſſary. And that it is moiſture only that it requires; for if it is moiſtened and ſhut up in cloſe veſſels, it equally anſwers as expoſing it to the open air, and no hydrogen is formed. In Mrs. Fulham's experiments it was very ſtriking: before ſhe could reduce the oxydes, moiſture was required. In forming nitrous air, it is alſo equally neceſſary. If nitrous vapour paſs through water, nitrous air is rapidly formed—(See Prieſtley, vol. iii. page 200*.) Alſo, upon mixing oxygen gas and nitrous air over water, the acid produced formed nitrous air again, very rapidly†. In the Galvanic experiment, it is very ſtriking; when the electric fire, after it has attracted the phlogiſton of the metals it paſſes through, attracts the water neceſſary to its aerial compoſition. Alſo in Mr. Henry's experiments, in paſſing the electric ſpark through the hydrocarbonated airs, and alſo the marine acid, it clearly (as he found) required water for the forming of air or gas. In ſhort, Chemiſtry is full of examples; but the moſt ſtriking are, the forming of airs by reducing the calces of metals with hot charcoal, and different airs are formed,—the cauſe of which I have already fully explained, and I hope ſatisfactorily; at leaſt I am certain I have done it to the candid mind. Beſides, I ſhould juſt aſk our *aerial* chemiſts, who are every day getting more extravagant with their abſurd flights, How could the nitrous air be formed in theſe experiments of Prieſtley juſt quoted? Could the water attract the oxygen from the nitrous acid, when it became nitrous air? — Anſwer this, ye poor deluded French votariſts! Of all the abſurd theories that have *hummed* mankind, this is certainly the moſt ridiculous: at leaſt it has been ſupported the moſt extravagantly and the moſt violently, and alſo the moſt diſhonourably; oppreſſing Juſtice, Honour, Reaſon, Common Senſe, and Truth. But, *Magna eſt Veritas & prævalebit.* The day of retribution will come.

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* "This will appear the more extraordinary, if it be conſidered, that water cannot be made to imbibe more than about one-tenth of its bulk of nitrous air. The production of it in this caſe, therefore, is quite another thing, and muſt have a different cauſe; though, had the quantity of it been ſmall, it might have been imagined, that the nitrous air from the biſmuth having impregnated the water, as, in ſome degree, it neceſſarily muſt, this nitrous air might have come from that ſolution.

"So great is this diſcharge of nitrous air, that if the impregnated water be left to itſelf, it will continue to emit air for a day or two; ſo that it is not improbable, but that it may, from firſt to laſt, yield fifteen or twenty times its bulk. On this account, if this water be confined in thin phials, it will endanger the breaking of them; and the ground ſtopples of ſtrong phials have often been thrown out by it with great violence."

† He ſays, "the ſaturation of water with nitrous acid from nitrous air, by means of dephlogiſticated air, makes a pleaſing experiment, on account of the great quantity of nitrous air decompoſed by this means at one pro-

The great body of our *aerial* chemiſts never queſtion this abſurd *aerial* fabric, built up to the very clouds,—

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ceſs, the quickneſs with which the decompoſition is made, and the viſible effect of the ſudden impregnation on the water. For the ſurface of it inſtantly becomes, as it were, oily, deſcending in waves from the top to the bottom of the water, while nitrous air iſſues plentifully from the bottom and ſides of the veſſel; a moſt remarkable phenomenon, of which a full account will be given in a proper place."

nay, even to the moon. They ſtrain their imagination to make it more ridiculous, if poſſible. All the bodies upon the earth are made of airs; even the very brains of our *aerialiſts*, animals, vegetables,—nay, minerals; even the mould which the vegetables grow in*!——But I am confident that they will not receive common ſenſe, without it comes in ſuch a form that there is no chink or loop-hole for them to eſcape out of, with their *aerial* flights! Now, I think I have them fixed in reſpect to the airs which their oxides give out upon being reduced, viz. fixed air and azote; and that they imbibe no fixed air or azote, but only pure air from the atmoſphere, as they calcine more quickly in pure oxygen gas.

Let us attend to Dr. Prieſtley's experiment on the calx of Mercury, with a little lead in it formed by trituration: See vol. iv. ſection 16. (The whole ſection will be too long to quote, therefore I muſt refer my reader to it.)—He found theſe metals would not be calcined without oxygen gas, and that this air was imbibed by the black powder. So that he ſays, p. 149†.

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* See an abſurd paper in the Philoſophical Magazine, by M. Otto.——I ſhould think Mr. Tilloch *even*, Mr. Kirwan, muſt be aſhamed of your collection in favour of Laputa: it even exceeds Mr. Nicholſon's Journal!

† "In order to obſerve the nature and progreſs of this operation to more advantage, I filled a glaſs phial, of about ten ounces, one-fourth part full of this mixture of mercury and lead; and inverting it in a baſon of the ſame, I agitated it with my hand, and preſently found that the air within the phial was ſenſibly diminiſhed, an evident proof that it was phlogiſticated; and in about ten minutes the diminution amounted to one-fifth of the whole, after which no agitation had any more effect upon it. Examining this air, I found, as I expected, that it extinguiſhed a candle. Indeed it was completely phlogiſticated, not being at all affected by nitrous air."

But when he expelled this air from this black powder, he found it principally fixed air and azote: See pp. 9, 11. where I have given the quotation. He alſo ſays, (page 150.) "When I agitated this amalgam in fixed air, nitrous air, inflammable air, or in any kind of phlogiſticated air, no black powder was produced, and thoſe kinds of air remained unaltered."—What a Herculean labour have I, to force open their eyes! It is allowed, that no air will calcine bodies but pure air. Let this air paſs through lime water, and incloſe it in a veſſel wherein ſome lead is placed to be calcined. At firſt the lead, by the action of the fire, burns to a grey calx. Then, by lowering the fire by a long proceſs of roaſting, minium, after that letharge all in the ſame air which had no fixed air in it; alſo what different airs theſe different ſtates of lead give back again.—See page 9, where a long quotation is made from Dr. Prieſtley's 4th volume, page 144. This then clearly proves, beyond all doubt, the air is changed in the proceſs, from pure air or oxygen gas, to fixed air and azote: this is incontrovertible. But I am tired with ſhewing that white is not black; it is too ridiculous to make any further obſervations.

O tempora! O philoſophia!

The Appendix to the Critical Review, alſo ſays: "A "ſtriking experiment was that in which the hydrogen "decompoſes the carbonated gas, by taking away its "oxygen."

Now, it appears that the fixed air is capable of being united with hydrogen, and forming this gaſeous oxyde; and need we be ſurpriſed at this aerial acid uniting to phlogiſton? Will not all the acids unite with phlogiſton, forming nitrous air and vitriolic acid air? Even the acetic acid will unite with phlogiſton—ſee Henry's vinegar. But M. De Saſſure's experiments clearly prove it. (See the Philoſophical Magazine.)——And another fact he obſerved, (which alſo clearly proves my theory), that when the electrical ſpark was taken in fixed air, there was ſome moiſture depoſited in the proceſs, which was clearly from this principle; as the fixed air, or aerial acid, got ſaturated with phlogiſton from the electric ſpark, it depoſited part of its water. So even in fixed air paſſing through hot charcoal, upon ſaturating itſelf with the charcoal, part of its moiſture will be precipitated and imbibed by the hot charcoal: all which facts are clearly proved from the well-known principles of chemiſtry. I forgot to mention that—the oxmuriatic gas, acting upon this gaſeous oxyde of carbon, burns its phlogiſton, or lets looſe its fire; and in conſequence, the fixed air it is united with is ſet looſe; but it is done in a very ſlow manner, very different from actual combuſtion with oxygen gas. But for a full explanation of their action upon each other, ſee my paper in the Gentleman's Magazine, for January 1802.

The Dutch chemiſts, it is ſaid, "approaching nearer to Dr. Prieſtley and Mr. Cruikſhanks than the French chemiſts, imagine it to be a compound of hydrogenous gas and carbon." Now theſe experimenters clearly find, that this gaſeous oxyde of carbon, beyond a doubt, is formed of an inflammable air. But that it is not formed of hydrogenous gas and carbon, is clear from firing it with oxygen gas. Mr. Cruikſhanks, by the combuſtion procured above double the quantity of fixed air, than the oxygen gas employed, and it required very little oxygen gas to burn it. Now, according to the opinion of the Dutch chemiſts, that this air is formed of hydrogenous gas and carbon; there ought to have been much leſs of fixed air produced in the combuſtion, as part of the oxygen would have gone to have formed water with the hydrogen; and, inſtead of a ſmaller proportion of oxygen being required for its combuſtion, there ought to have been a greater proportion. And alſo, as Mr. Cruikſhanks obſerves, they produce no water in their combuſtion, when, according to the opinions of the Dutch chemiſts, they ought to produce a great quantity. Therefore my reader muſt be ſtruck with this conviction, that all their attempts to account for the phenomena on the French theory, are truly abſurd; but with mine, nothing is more rational.

I have clearly proved, that the alkaline ſalts being made cauſtic, is by fire expelling their fixed air, and ſupplying its place in the ſalts: therefore cauſtic ſalts are formed with the fire they received from the lime. Now it is well known, that the cauſtic volatile alkali, or pure ammonia, will calcine and diſſolve copper. But this proceſs is performed by the fire of the alkali acting upon the phlogiſton of the metal, aided at the ſame time by pure air, and by producing a ſlow combuſtion. If this proceſs is conducted in a glaſs bottle, filled either with atmoſpheric air or pure oxygen gas, (both previouſly paſſed through lime-water), all the pure air will be imbibed by the mixture; part of the oxygen gas entering the calx of copper, and another part entering the ammonia, making it carbonated. From whence, you vague theoriſts! comes the charcoal in the proceſs? I ſhould wiſh the tyro to perform this ſimple experiment, and reaſon for himſelf, and be no longer the dupe of this vile combination.

The experiment of the French chemiſts in firing hydrogen and oxygen gaſſes, producing, as they ſay, only water, and nearly in weight to both airs. Now, only conſider how complicated experiments of this kind are; the accurate knowledge of the weight of the airs conſumed, &c. But the moſt material motive is, the French vanity in giving birth to a revolution in chemiſtry, and alſo the experiment being performed with great pomp before their king. Therefore, as we have ſeen the great errors, in ſome of the common experiments, to favour their theory, can we be ſurpriſed at their errors in this? But to the man of common ſenſe I addreſs myſelf: is it not more likely there are ſome errors in the experiment, than that ſuch an extraordinary phenomenon ſhould take place, as water being formed of theſe airs. Before Nature forms her works, ſhe muſt have the materials: then conſider the immenſe body of waters upon the earth, and conſider the immenſe volume that theſe airs would fill before the water was formed: then what would contain them, as they are not to be exhibited in their ſeparate ſtate, but in the form of airs. But only conſider this fact, (as Mr. Paul obſerves, ſee page 74.) That hydrogen "obtained by means of ſulphuric acid, water, and iron, "in a humid way, it has a different flavour; ſomewhat "hepatic." Then in firing this hepatic air with oxygen gas, in a ſlow combuſtion, only water is produced. Is it not evident, then, that the vitriolic acid in this air, by the combuſtion eſcapes? But clearly the way to aſcertain the experiment moſt accurately, is to perform it by one exploſion upon the airs, without working whole days upon the combuſtion; and when done in this manner, there is a great reſiduum of an acid water, and alſo it is leſs in weight than the airs conſumed.

Mr. Fourcroy, that wonderful aerial chemiſt, in his moſt wonderful theory of animal life, ſays, that animal ſubſtances conſiſt principally of azote; and the great experiment to prove this is, that the nitrous acid produces azote with animal ſubſtances. But Dr. Prieſtley, in his 2d volume, ſect. viii. ſhews that he procured with ſtrong nitrous acid and animal ſubſtances, nitrous air, fixed air, and inflammable air—nay, even a kind of common air.—Speaking of the air from the nitrous acid and ſalmon, he ſays, (page 150) "The air, in all the ſtages of this experiment, was in part fixed, precipitating lime in lime water. In the middle of the proceſs, the reſiduum was nitrous; but only in a ſlight degree. Towards the concluſion it had no ſenſible effect on common air; and at laſt it burned with a blue lambent flame, which continued a conſiderable time after I had withdrawn the candle by which it had been ſet on fire. In the air that came juſt before the laſt, a candle barely went out, ſurrounded by a ſlight flame of that colour."

Alſo, (page 151.) "Air produced from a number of waſps, diſſolved in ſpirit of nitre, was partly fixed, and the reſiduum ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2½ meaſures. When the flame of a candle was dipped into it, it burned with a greeniſh lambent flame."

But moſt of the French experiments are to be doubted. If the nitrous acid is applied to moſt bodies, it is well known that air is procured from the mixture, which they ſay is from a decompoſition of the acid. But when applied to animal ſubſtances, the air procured is not from the acid, but the animal ſubſtance, they ſay.—*Laughable in the extreme!*

But that not only nitrous acid, but all other bodies formed of it, may be formed into what they call azote, Dr. Prieſtley ſays, (vol. iii. p. 328.) ſpeaking of dephlogiſticated nitrous air*.

But they force their theory to turn the nitrous acid into all ſhapes, and to be formed of their wonderful elements.

Mr. Fourcroy being beat from his former hypotheſis, ſeeing how very abſurd I had made it appear; therefore he and his coadjutors muſt produce ſomething to keep up the *hum*, or *gull* the credulous multitude, the *ſervum pecus*: and he adopted this abſurd hypotheſis, taking his opinion from theſe experiments of Dr. Prieſtley. We will more accurately inveſtigate them. The Dr., prone to hypotheſis, had at firſt adopted an idea that vegetable ſubſtances yielded nitrous air with the nitrous acid, and animal ſubſtances azote; but by continuing his experiments he greatly relinquiſhed the idea, as we ſhall ſee in our examination. In page 154, he ſays†:

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* "No leſs remarkable was the reſult of heating malleable iron in this air. For the bulk of the air was increaſed, and yet afterwards no part of it was imbibed by water. It was all phlogiſticated air, without any mixture of fixed air. It ſeems, therefore, as if this kind of air wanted nothing but phlogiſton to make it phlogiſticated air, and nothing but the principle of heat to make it dephlogiſticated air.

"This experiment happening to draw my particular attention, I repeated it very often, and always had the ſame reſult, though with variations, depending on the quantity of the air, and the heat I was able to apply."

† "Both the *white* and the *yolk* of an egg, which I tried ſeparately, yielded a conſiderable quantity of air, when diſſolved in ſpirit of nitre, and the difference between them was not ſenſible. In both caſes part of the air was fixed, precipitating lime in lime water, and the reſiduum was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2½ meaſures."

All oils or animal fat, yield, according to the Doctor, with the nitrous acid, only fixed air and nitrous air. He alſo ſays, page 157*:

When ſpeaking of the air he got from the nitrous acid and charcoal, cork, &c. he ſays, (page 140): "With a "moderate degree of heat the air was ſtrongly nitrous; "and with a great heat the air was turbid, and much "leſs nitrous." Now we ſee how the produce varies, according to the heat applied; and this confirms my doctrine, that great heat makes the acid more intimately unite with the phlogiſton of theſe bodies, and expand them into a more light air,—juſt the ſame as in that experiment of the Doctor's,--(ſee page 92, where I have quoted it), the nitrous dephlogiſticated air, as he calls it, turning into azote. Alſo, that theſe different airs are not elements, but only the acid united to phlogiſton; for by diluting the acid he got a leſs nitrous air, the acid being not ſo ſtrong, and does not ſaturate itſelf with ſuch a high ſaturation of phlogiſton. The ſame with vegetables. But I ſhall refer my reader to the Doctor's book. —In accounting for the nitrous air from ſalmon, he ſuppoſes it might be owing to its turning to charcoal from being dried. My reader will no doubt ſmile! If I may be allowed to argue a little, to introduce a little common ſenſe into our philoſophical reaſoning; all animal ſubſtances putrify in the air; this is, they ſay, from the oxygen that the air contains; animal life ſubſiſts by breathing, this, they ſay, is from the oxygen the air contains; animals live upon vegetables, which they ſay poſſeſs no azote, and none is received in reſpiration; from whence, then, is this great animalizer received, you half-witted philoſophers?——*Go to bed, go to bed,* and let the world be no more peſtered with ſuch abſurdities!

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* "From part of the brain of a ſheep, diſſolved in ſtrong ſpirit of nitre, I got a quantity of air, about half of which was fixed air, precipitating lime in lime-water, and the remainder was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2¼ meaſures. When it was completely diſſolved, and by a ſtrong heat, the air came over very turbid, and a candle burned in it with a lambent greeniſh flame.

"I repeated the experiment with part of the ſame brain that was *boiled*, and with the ſame reſult; except that I did not continue the proceſs ſo long. The reſiduum of this air, when the fixed air was waſhed out of it, was ſo much nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2 1-5th meaſures. This I tried with the laſt of the three portions of air that I took. The firſt and ſecond were not ſo highly nitrous; and yet I am confident that all the three portions were wholly the produce of the ſolution, both the phial and the tube being filled with bubbles, in the form of froth, before I took any air at all.

"A piece of boiled mutton, diſſolved in very ſtrong ſpirit of nitre, yielded air, which was partly fixed, with the reſiduum ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2⅓ meaſures. Diſſolving a quantity of the ſame mutton, in the ſame ſpirit of nitre, diluted with an equal quantity of diſtilled water, I procured air, which was not half ſo much nitrous as that in the preceding experiment. With the ſame reſult I alſo made this experiment with the white of an egg, which gave air much leſs nitrous when diſſolved in a diluted ſpirit of nitre, than in the former caſe."

Mr. Thenard, upon vinous fermentation, could not detect any azote in vegetables, though ſought for with the greateſt attention. But all that pomp upon Lavoiſier's theory, which he diſplays, and which he thinks ſuch a a national honour, I flatter myſelf is not correct, but muſt be given to England through my labours, though the debaſed combination, Reviews and Journals, want to tear it clandeſtinely from us.

In the next ſection the Doctor ſays, (page 161) "The "celebrated Mr. Stahl obſerved, that by diſtilling iron in

" the marine acid, ſome nitrous acid was produced. Mr.
" Woulfe informs me, that he had done the ſame by a
" proceſs different from that of Stahl; and alſo that he
" has converted the acid of nitre into the marine acid,
" which I believe was never done before.——This I con-
" ſider as a capital diſcovery; and whenever this excel-
" lent chymiſt ſhall think proper to publiſh his proceſs,
" it will, I doubt not, be a great means of advancing the
" bounds of natural knowledge."

The above, with many other arguments that might be advanced, ſhew that the acids may be changed. This is proved in fifty inſtances that might be named.

Mr. Watt, in the Philoſophical Tranſactions, vol. 74, p. 348, ſays, " And by the experiment I have mentioned " of Dr. Prieſtley's, (6) it appears that nitre can produce " one half of its weight of dephlogiſticated air." And Dr. Prieſtley ſays in the ſame volume, p. 297*.

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* " At Mr. Watt's requeſt, I endeavoured to aſcertain the quantity of acid that was expelled from nitre, in procuring the dephlogiſticated air from it. To do this, I put two ounces of purified nitre into a glaſs retort, and receiving the air in 300 ounce meaſures of water, only filled each recipient half full, and agitated the air very much in the water, in order to make the water imbibe as much as poſſible of the acid that it contained. However, notwithſtanding this agitation, every veſſel of the air retained a ſtrong ſmell of the acid. The moment the air ceaſed to come, I filled a large phial with the water, and carrried it to Mr. Watt, who carefully examined it; and in a paper which he preſented to the Royal Society, and which is publiſhed in the Philoſophical Tranſactions, he has given an account of the quantity of acid that it contained, computing from the ſpecimen that I brought him the quantity that was contained in all the 300 ounce meaſures of water; from which it may be fairly inferred, that there was no occaſion to ſuppoſe that any of the acid entered into the compoſition of the air, but that it was all either rendered volatile, or retained in the water."

Now, what proportion muſt the azote have formed in the nitrous acid of the nitre? Mr. Lovoiſier and De la Place ſay:

" Pot-aſh	7 oz.	6 gros	51.84 grains,	=	4515.84 grains:
Dry acid	8	1	21.16	=	4700.16

The above quantity of dry acid is compoſed of

Oxygen	6 oz.	3 gros	66.34 grains,	=	3738.34 grains.
Azote	1	5	25.82	=	961.82."

But even *they* are obliged to acknowledge, that this ſuppoſed formation of the nitrous acid is not confirmed by their own experiments, and what Mr.Fourcroy is obliged to ſay in his Philoſophy of Chemiſtry, (ſee page 8 of this pamphlet), — I have endeavoured to bring all the great maſters or founders of this French theory, and their experiments upon which they founded their opinions, into a group.

But any of the acids may be neutralized and aerialized in the ſtate of what they call azote, as Doctor Prieſtley ſhews that fixed air by heat, and the fixed fire of iron, became an azote. But azotes will be found to differ according to the acid they are made from. As fixed air, when ſaturated with fire, produces a different kind of what they call oxygen gas than that made from nitre; and alſo that made from nitrous air differs from both,— namely, that air which Dr. Prieſtley calls dephlogiſticated nitrous air, and which air is not reſpirable, but only capable of making our wiſe theoriſts feet, dance, and caper like *Merry Andrews* to a French cotillon, while it makes their heads roam at large into their *aerial* regions, their ſenſations being moſt ecſtatic, (and I ſhould ſay, *lunaſtic;* or, ſtill more apropos, *Laputaſtic.)* — See Dr. Beddoes and Davy's Reports. No ſtage quack ever exhibited ſuch Wonders! WONDERS!! WONDERS!!!

The French theory ſays, that this conjurer, *Oxygen* forms metallic oxides, and that fixed fire has nothing to do in the proceſs, which appears equally abſurd with their other doctrines. Mr. Prouſt has clearly ſhewn, after me, that ſulphur unites to metals, and that oxygen has nothing to do in the proceſs, as in pyrites. (See Nicholſon's Journal for February 1802.) Alſo in cinnabar; it is well known that in making of it, the mercury poured into a retort with melted ſulphur, produces a moſt active inflammation. Now I only aſk medical gentlemen, if the mercury in the cinnabar is not in the ſtate of calcination? It is well known, that mercury, in its metallic ſtate, is perfectly inactive in the ſyſtem; THEN IS CINNABAR SO? No: as Mr. Prouſt ſays, Mercury has a ſtronger attraction for ſulphur than even the conjurer, *Oxygen**. But know this: Various bodies calcine metals as well as oxygen; as, all the acids, water, ſulphur, alkalies, ſpirits of wine, &c. &c. and that this conjurer has nothing to do in the buſineſs, as I have clearly proved. And, reader, if thou art not completely ſatisfied that this theory is perfectly abſurd, and unable to explain the phenomena, after what I have clearly proved in my different publications, I can only ſay, thou art either nature's fool, or the fool of this baſe combination.

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* Mr. Chevenix's experiments prove, that the fixed alkalies calcine even platina; and ſurely the moſt prejudiced chemiſt cannot ſay that oxygen has any thing to do in the proceſs. Mr. Prouſt ſays, "Artificial cinnabar conſtantly yields 85 per cent. of mercury on the one hand; on the other it relinquiſhes 14 and 14½ of ſulphur to the antimony which is uſed for effecting its decompoſition. The mercury is not therefore oxiginated in cinnabar." He alſo ſays, "Mercury poured into a retort with melted ſulphur, always occaſions an inflammation; but if both have been previouſly heated, the union takes place, and the æthiops afford cinnabar without flame. As to that which accompanies the proceſs uſed by the Dutch, it deſerves to be better examined than has hitherto been done."—Now here it appears that mercury poured upon melted ſulphur, produces inflammation, and this inflammation is induced without oxygen, as the cinnabar has no oxygen. Therefore it muſt be performed, as I have all along proved, by the earth of the mercury attracting the ſulphur, and which chemical action ſets its fixed fire free as actual fire, the ſulphur ſupplying its place, the ſame as oxygen gas does; or, more properly, the fixed air and water of the oxygen gas. The Dutch experiments here alluded to, were firſt made by me, though no Chemiſt or Reviewer would do me the juſtice to give them to me.

What muſt thou think at the baſe artifice they uſe, not even mentioning my name or my experiments, though often adopting them; — that they have got hold of all the hackney writers, traders and purchaſers in ſcience; alſo the journaliſts and reviewers, and all other periodical publications, their machinations extending to a regular ſyſtem. If they knew their theory ſtood upon the ſolid baſis of immutable truth, why not court inveſtigation? Now, I call upon thee, my reader, whoever thou art, if thou art ſtill infatuated with the French theory, and err from opinion, then come boldly forward, and ſtate thy objections to my theory in a manly way, and not as this combination does, by every low artifice to ſuppreſs diſcuſſion. I am but an individual: then aſk thyſelf, if they were not ſenſible of the ſoundneſs of *my* ſyſtem and of the perfect abſurdities of *their*'s, they would have moved heaven and earth as it were, against its propagation and candid diſcuſſion. No; they tremble when it is named: their behaviour has been ſo ſhameful in the extreme, that when the truth comes to be fairly canvaſſed, they must appear *low indeed!* They may ſend their agents from London to France, and even over the continent; and from Dublin and Edinburgh, to London, to conſult how to depreſs my ſystem: it will not all do; Truth *will* and *muſt* prevail; the public must open their eyes.

Fixed air is ſaid to be formed of carbon and oxygen; but as even Mr. Cruikſhanks obſerves, that the fixed air formed is never equal to the weight of the oxygen and carbon conſumed, obſerve what the immortal Scheele ſays, page 48. Mr. Lavoiſier, that theoretical chemist, ſeriouſly propoſes a manufactory to decompound the fixed air of the calcareous earths into carbon; and our Engliſh chemists, the ſervile followers of the French, ſuppoſe they have performed this wonderful decompoſition by the aid of phoſphorus. But I have before ſhewn the abſurdity of theſe experiments; I ſhall ſay nothing further here, but only quote a paſſage in Nicholſon's Journal, by Citiz. Steinacher (from *Annales de Chimie;)* and alſo refer to Mr. Prouſt's experiments, (page 132), who "has informed the public, that, in the distillation of phoſphorus, a combination of this ſubstance with the charcoal constantly took place. This important diſcovery extends much farther than its celebrated author has ſhewn."

" Take the most brilliant and most tranſparent phoſphorus, which has not only been strained through chamois leather, according to Woulfe's method, but has alſo been diſſolved ſeveral times in nitro-muriatic acid, as done by Count Muſſin-Puſchkin, or which has been treated with oxigenated muriatic acid, after the mode of Mr. Juch of Wurkburg; let it be heated gently in a long ſlender tube; red parts will ſeparate from it. Put a few grains of this phoſphorus, which is conceived to be ſo pure, on a ſilver ſpoon, and ſet fire to it; a red trace will remain. If the ſpoon be heated in the dark, the red trace will be ſeen still to burn, and a coal will remain impregnated with phoſphoric acid."

Alſo Mr. Muſket's experiments upon iron clearly ſhew the abſurdity of Mr. Clout's and the French philoſophers opinions and experiments, making them out both erroneous and abſurd. The leading experiment that made them adopt this abſurd theory, was the firing of oxygen gas and inflammable air producing water. This experiment Mr. Cavendiſh is ſaid to have firſt performed, tho' the French philoſophers claim it. But the truth is, that upon my publication in 1781, I there clearly prove, that fixed air, water and fixed fire, form atmoſpherical air;—that water enters into the compoſition of airs, as an eſſential part of their compoſition, being, as I ſtile it, the water of compoſition. After this, the different chemiſts in trying my theory, found the water which I had clearly proved to be by different experiments, always depoſited when atmoſpherical air is burned. But to differ from me, they endeavoured to eſtabliſh a theory that oxygen gas and combuſtible bodies (that is, inflammable gaſſes) form water. That the acid which undoubtedly oxygen gas contains, is not always found, I clearly proved that if the inflammable gas, or phoſphorus and ſulphur, (being formed of a great quantity of fixed fire) are burned, the combuſtion will be ſo intenſe as to decompound the aerial acid from its aerial ſtate. But if the combuſtion of the heavy inflammable gas, and inflammable bodies, be not ſo intenſe, they will leave the acid of the pure air in its aerial ſtate as fixed air.

But when the combuſtions of oxygen gas and hydrogen gas are ſlowly conducted, the pure air's acid will eſcape with the fire ſet looſe. I proved this by paſſing the electric ſpark through the nitrous acid; by repeatedly paſſing it through, it in time took along with it the acid. This fact at firſt made our wiſe philoſophers wonder, but it is now moſt ſtrikingly confirmed by the Galvanic experiments *.

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* Dr. Black, in ſpeaking of the French theory, ſays, "I affirm that "even to a philoſopher, the proofs of the fundamental propoſitions which "have been acquieſced in by the authors of this arrangement, are very "ſcanty, very ſlight, and very refined." I ſay, they are very prepoſterous, very abſurd, and very ridiculous.

As there is ſuch a rage for new nomenclatures, allow me to form one which I do poſitively declare will be handed down to poſterity, while their's will be damned by the ſame tribunal. Modern chemical philoſophy, I call *puerile vanity, and folly.* Modern chemical philoſophical integrity, I call *injuſtice.* Modern chemical philoſophical candour, I call *deceit.* Modern chemical philoſophical *eſprit de corps* of their junto, *the baſeſt perſecution of true ſcience.* Modern philoſophical combination, *the baſe aſſaſſins of truth and its friends.* Modern chemical philoſophical Reviewers and Journaliſts, *the proſtituted agents of this combination.*——Such is the NOMENCLATURE I give to the French philoſophy, which has ſo long *gulled* the credulous world, and, with all my baſe and mean opponents, ſhall be handed down to the remoteſt ages, (along with my important truths), againſt all the ſelfiſh and baſe arts of my enemies,—who are alſo the enemies of true ſcience, as they have ſo baſely endeavoured to ſuppreſs them. Future ages will there behold conduct unparallcled in the hiſtory of letters. There has been many a fooliſh theory conſigned to oblivion; but this French one ſhall *live*, in recording their baſe behaviour to me. Names that thought they would be immortalized for their diſcoveries, *ſhall be immortalized.* BUT HOW? . . . .

I wonder when John Bull will have this French ſcientific film taken from his eyes. The gentlemen of the combination can beſt tell, as they have been the principal means of fixing it there. I flatter myſelf that his own ſolid ſenſe and liberality of heart, will not only tear this ſcientific aerial film from his eyes, but that he will tear from them all the films of the French revolutionary principles, and ſtick cloſe to his King, his Country, his Conſtitution, his Religion, and his God; and oppoſe all the French revolutionary doctrines, both ſcientific, politic, deiſtic, and atheiſtic.

In applying this French theory to animal life, nothing can be ſo truly ridiculous. In my former publications I have ſhewn this in ſtrong colours: (ſee my laſt letter to Mr. Cavendiſh). But as our philoſophers are beat from one abſurdity, they take refuge in another. I ſhall give the laſt hypotheſis, juſt imported from the French of Fourcroy by the Reviewers. The Critical Review (Appendix, vol. xviii. p. 521.) ſays: "From this quantity of "azote our author conſiders the urinary ſecretion as de-"ſigned to ſeparate the ſuperabundant azote, as the "lungs ſeparate the ſuperabundant oxygen, and the liver "the ſuperfluous hydrogen."—Moſt of my readers will ſay, this muſt be ſome miſtake; the lungs' office cannot be for the *diſcharging* of oxygen; when all have hitherto agreed that the lungs' important office was to *receive* oxygen. As to my part, nothing ſurpriſes me, for I had ſhewn the extreme errors of their former opinions, by which they were forced to this explanation of the phenomena, That the oxygen gas was not for ſouring the ſyſtem, as they ſay is its great operation in combuſtion. As metals, for inſtance, in the proceſs of burning; our thick-headed philoſophical forefathers, and all the vulgar, thought that the act conſiſts in letting looſe the fire of the metal, or combuſtible body. No:—Our great, wonderful, ſurpriſing French and Engliſh philoſophers of the 18th and 19th centuries, ſay, It is the act of ſouring the metal or combuſtible body. So coals, charcoal, &c. in burning, is not a burning proceſs, but a ſouring proceſs. So our preſent chemiſtry is not an enlightening one, giving either light or heat; but a *darkening*, *ſouring one*. What profound *ſouring*, *black* and *dark* diſcoveries! I wiſh Swift had been living to celebrate them. I think his flying iſland would be unworthy to have the honour of ſuch inhabitants. No—he muſt have found another iſland for ſuch *darkening* and *ſouring* philoſophers.—Their province now is, by all the dark chicanery human art is capable of, in *darkening* and *ſouring* the public mind, to my great, enlightening and important diſcoveries;—an act in uniſon, and worthy of the authors of theſe *ſouring* and *darkening* doctrines, but will one day receive its proper reward; they will be doomed to everlaſting darkneſs, but not to oblivion—their *ſouring* doctrine will follow them, and *pickle* them, for a beacon, *in terrorem*, that future philoſophers may avoid their dark machinations and conduct.

This *ſouring* doctrine appears very extraordinary in putrefaction. Bodies become alkaline when they putrify, *and not ſour*, by imbibing oxygen; they ſay. Nay, more wonderful; the part of the oxygen gas that is not imbibed by the putrid body, is turned into an acid, as fixed air. Alſo their theory of combuſtion appears here very ſingular.—Wood, though it poſſeſſes (according to them) ſuch a quantity of carbon, will not affect the air; but fleſh, which poſſeſſes ſo little carbon, being all azote, does not turn the oxygen of the air to the nitrous acid, but turns it into fixed air, and that ſo rapidly, without receiving any increaſe of temperature, or producing any heat; for Dr. Monro found none, upon placing a thermometer in a cadaverous whale: and this cadaverous whale, that was injuring the air all around it for miles, and for months, turning the pure air into fixed air (the ſame as the lungs in the living animal) had no living lungs and circulation to diſcharge the carbon, which (they ſay) is the office of the lungs. Alſo the fetus in the womb; its blood is artracting the fixed fire neceſſary for its animal life from the blood of the mother, by the *placenta*. Then, according to the French ſyſtem, what becomes of its carbon, as it has no functions to diſcharge its fixed air? Anſwer me this, you French votariſts! But how beautifully does my ſyſtem account for it. The animal fleſh attracts the fixed fire of the air; the fixed fire that neutralized the fixed air of the pure air, and in conſequence the animal fleſh, becomes a high ſaturation of fixed fire, called *ammonia*. But in the living animal this fixed fire, received from the air by its blood, &c. the circulation does not allow it to accumulate, ſo as to form ammonia; but ſets it looſe, as free fire producing animal heat. And in combuſtion, the burning body does not accumulate it, but it is ſet looſe as free fire.

Fourcroy, Berthollet, &c. were driven to this azote ſyſtem, to account for the animalizing principle. They at firſt ſaid, oxygen formed the animal fibre, and alſo animal oil. But M. Caradori ſhewed, by experiments, the abſurdity of thoſe opinions; ſtill they pertinaciouſly adhere to their other element, azote. They are determined to adopt any abſurdity, rather than give up their high-flown aerial hypotheſis. It is juſt the ſame with Bartholket's prediſpoſing affinities, to reconcile the groſs contradictions in their table of attractions. But, like other chemiſts, I am fond of a new nomenclature: therefore I call Mr. Bartholet's prediſpoſing affinities, *prediſpoſing follies!* to *gull* our wiſe theoriſts. There never was ſuch vague reaſoning thrown together ſince chemiſtry exiſted. My reader muſt excuſe me in analyſing them: I have already had too many laborious hours upon his account.

And now the urinary functions are to diſcharge the azote, and the liver the hydrogen. What ſurpriſing miſchief theſe aerial bodies do in our animal ſyſtem! as much miſchief as they have done in our philoſophy.—But cannot theſe imaginary beings be chained down by common ſenſe? Thoſe animals that live upon the pureſt oils, and drink nothing but pure water. As one pound of oil, they ſay, is formed (according to Mr. Lavoiſier) of "12 oz. and 5 grains of charcoal, and 3 oz. 2 gros, 67 grains of hydrogen," then how come animals to be ſupported by theſe bodies? How comes the Greenlander's whale oil to ſupport him, or the olive oil and butter of Europe ſupport its inhabitants? . . . They can form no animal fleſh, as they poſſeſs no azote; the nitric acid would be the proper food, or animalizer. And yet it is ſtrange to be told, Mr. Fourcroy, and your gaping cre-credulous Engliſh admirers, the Greenlander's urine is really *ſtronger*, and has more of this azote or ammonia in it, than thoſe animals that feed upon fleſh, their perſpiration and urine being moſt offenſive. When I treated upon putrefaction, the oxygen gas or ſouring principle producing putrefaction, I thought moſt extraordinary; and was for having theſe advocates for ſuch abſurd doctrines ſwallow a rotten egg, by way of putting a little common ſenſe into them. Now I am for having the advocates of the azote ſyſtem to ſwallow a little of the urine of a Greenlander, for the ſame purpoſe: I ſhould think that ſuch food as the egg and urine are adapted to cleanſe this combination of their diſgraceful, baſe, and mean behaviour to me. Such conduct requires ſtrong remedies to cleanſe and purify.—As to the liver being the ſecretory organ for hydrogen, as water is the body which contains ſo great a proportion of it; then drinking water would furniſh the liver with the greateſt proportion of hydrogen. But they never have had the audacity to ſay that water is decompounded in the ſyſtem; but they ſay it is decompounded in far more ſimple proceſſes — putrefaction, vegetation, and metallic calcinations, alſo in the ſimmering or boiling of vegetables in water, &c. nay, in that common proceſs of evaporation. Then why not in the animal functions, which certainly produce greater chemical changes in bodies than any other proceſſes; which all phyſiologiſts allow. According to this French theory, the greateſt wonder is, that the urine of animals is not formed into French brandy; for water being formed of oxygen and hydrogen, and theſe two bodies, with the addition of charcoal, form ſpirits, (or brandy). Therefore, as animals take in ſuch a quantity of carbon, living upon graſs, and drinking plentifully of water, the moſt rational conjecture is that their urine ſhould be formed of brandy, and not of azote; and how the *Diable* can our French theoriſts find out by what means the animal can receive all this azote, as not only to form his fleſh and ſubſtance, but to be in ſuch abundance as to require its conſtant diſcharge? One would ſuppoſe, according to the ſame enlightening theory, that he muſt feed upon the nitric acid, as that would directly agree with all the diſcharges. The lungs to diſcharge the oxygen of the acid, and the urine the azote.— Juſt the thing itſelf!

What produces that accumlation of bile and liver complaints in hot countries? It cannot be from eating animal fleſh, for they live principally upon vegetable food; but in cold countries, chiefly upon animal food.—Now, you *aerial theoriſts!* (to introduce a little common ſenſe amongſt you), it is owing to the immenſe heat or fire there, which is conſolidated or fixed into their animal juices*; and from this cauſe putrefaction is accelerated there.

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* Count Rumford ſays in his Eſſays, that the boiling of vegetables in water decompound the water, and by that means increaſes the nouriſhing food of the contents of the pot. I ſhould ſeriouſly adviſe the Count, for the future, to let the pot boil quietly, and alſo to eat his apple-pie quietly, even though it ſhould *burn* his mouth. As his philoſophical diſcoveries and reflections upon them may *burn* his reputation as a philoſopher—and as he is ſo much puffed up with that reputation, one burn will be more ſeverely felt than the other. His apple-pie produced diſcoveries (as he thought) capable of explaining how Nature produced her greater operations—nay, even how animal heat was formed. Indeed it was a moſt fortunate and wonderful apple-pie, and a ſtill more fortunate and wonderful COUNT.—See his Eſſays, and alſo his Letter to Mr. Pictet.—And all theſe from APPLE-PIE! Diſcoveries enough to deify them both, with the aſſiſtance of the boiling-pot. O ſweet apple-pie! ſuperior to Newton's apple; thou art immortalized! Children have often burnt their mouths from eating of apple-pie; but no child ever ate of THEE to ſo important a purpoſe. An old child in ſcience has aſſumed a gigantic ſtature. As for thee, O Reader! thou haſt often eaten of apple-pie, but thou wanted the Count's brains to immortalize the diſh. The Edinburgh College has juſt given ſuch another delicious French apple-pie to the public, which they call their PHARMACOPEE, which is ſo very nauſeous and groſsly ſweet, that the proſtituted and mercenary declaimers in favour of the French philoſophy, the Reviewers, are ſick and cloyed with it; as they ſay it is "out-Heroded Herod!" But I ſay, even out-Rumforded Rumford. His example is nothing even to elderly children, or elſe they might have taken warning by him. But they not only have burnt their own mouths, but they are anxious to burn thoſe of the public, by cramming this French pie down their throats. But, reader, I am afraid thou wilt ſay they are very ſweet pies. But I have given thee enough of them, conſign them to oblivion; but I hope not until thou haſt given them a proper *ſauce* as a correcter,—as too much *ſweeties* injure children, and make them require the rod.

Reader, laugh not at this PIE of PIES. For from burning the Count's mouth, it produced reflections ſo *deep*, *wiſe*, and *wonderful*, as to procure

France is equally anxious to outrival England in ſcience as well as arms, being her determined enemy: then what do thoſe Engliſhmen deſerve, who by all mean and baſe arts endeavour to exalt the erroneous theories of the French, and ſuppreſs the truth of my diſcoveries? And why, Engliſhmen, are you thus gulled by their baſe and vile arts? What muſt you ſuppoſe, when not one of them dare enter the conteſt, but ſkulk, depending upon the falſe impreſſions they have given you—knowing that, if they were to come fairly and openly forward, defeat and diſgrace would attend them and their baſe and vile combination. I have challenged—nay, *coaxed* them to the combat: and, another gentleman and myſelf offered a medal of the value of ten guineas upon the ſucceſs of any one of them, who would fairly controvert my theory. But all to no purpoſe.——Since my firſt publication in 1781, I have ſilenced a number, (of different ſets)—Mr. Cavendiſh, Mr. Kirwan and others, were amongſt the foremoſt of our *aerial experimenters*; they were ſoon driven from the field, giving room for others, all of which

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him his own medal from the Royal Society. I ſhould ſuppoſe that future ages will rank this adjudgment with Juſtice Midas's.

The Count received great honour from his ſuppoſing that in firing gunpowder, the exploſion was owing to the expanſion of water. But in this opinion he never once hinted at mine, which I had publiſhed many years before in the Gentleman's Magazine. But I did not carry my opinion ſo far as him as to ſuppoſe it was the ſole cauſe of the expanſion—nay, the principal one is the expanſion of the nitrous acid; but I ſuppoſed that the water in the nitre might likewiſe aſſiſt. And I ſhould have thought that when he was upon his experiments of the pure metals being ſo eaſily reduced by phlogiſtic bodies, he ſhould have likewiſe mentioned my experiments upon the ſame ſubject.——But it is endleſs to enumerate every part of their behaviour to me.

diſappeared in their turn. The late ones were Dr. Pearſon, Mr. Cruikſhanks, Mr. Henry, jun. Dr. Wells, &c. and they alſo have diſappeared in their turn. Now I think the preſent experimenters are—Mr. Cuthbertſon, Mr. Cavendiſh's inſtrument-maker; Mr. Accum, the practical chemiſt to the inſtitution: — alſo Mr. Davy is dragged forward to contradict his former experiments.

Charcoal (they ſay) is formed of carbon, and diamonds of the pureſt carbon, without any alloy:—how comes it, then, you conjurers! that oxygen gas does not attract their *carbon*, there being nothing to reſiſt it? Animal fleſh acts rapidly upon oxygen gas, injuring it, and turning it into fixed air; (and we are taught to believe that it poſſeſſes little or no fixed air, and what it does poſſeſs is chemically united with other bodies: therefore it muſt appear ſtrange to common ſenſe, that it ſhould act upon the atmoſphere in any temperature, and diamonds *not*.) But, ſtill more ſtrange! animal fleſh will not inflame, while carbon will. If the former's carbon is eaſier attracted by the atmoſphere, why will it not inflame? The carbon, after injuring the air by turning it into fixed air, almoſt diſappears totally, having very few aſhes; but the animal fleſh does not diſappear, but turns to ammonia, a ſalt formed of fixed fire—and, ſtill more ſtrange! the one producing an immenſe degree of heat; the other none. Then need I hammer it into the Frenchified chemiſts, that the one, (the burning the carbon) is an act of looſing a quantity of fire, and that the other, (the putrefying fleſh) is an act of accumulating it. If he diſputes it, let him apply his *noſe* to both proceſſes, and he will be confirmed: for in one caſe it will be *burned*, and in the other it will be *ſtunk*. Certainly this kind of *demonſtra-*

tion will penetrate *his head;* but I am afraid it is not his *head*, it is his HEART now to be converted. I cannot ſuppoſe any man, whoſe *head* is formed of the *commoneſt materials*, but muſt be convinced.——When upon the gaſeous oxyde of azote, I forgot to mention what Doctor Higgins ſays upon it, page 550*.

The Doctor alſo ſays, (page 163), "From the proceſs of theſe experiments, I did not heſitate to conclude, but that which is called dephlogiſticated nitrous air, is common nitrous air deprived only of a portion of its dephlogiſticated air."

Now, need I enlarge upon this? I ſhall only ſay, If we are not to kick common ſenſe quite out of doors, both theſe theories are abſurd. Doctors Prieſtley and Higgins give experiments which prove, that iron, expoſed to nitrous air over mercury, &c. when the air is reduced one-third, that this gaſeous oxyde of azote is produced; and when two-thirds, azote. Probably it may be ſaid, that water, that never-failing auxiliary, is decompounded in this proceſs. If ſo, what becomes of its ingredients? Its oxygen would go to the calcination of the iron; but if a part was attracted by the nitrous air, they would form the nitrous acid; and what becomes of its hydrogen, *I aſk**. If the iron is examined, it is a pure calx of iron and the nitrous acid. But if the proceſs is continued for many months, the iron turns it into azote.—Dr. Higgins ſays, "I let the other tube ſtand, until the air contracted to "more than one-third of its original bulk; the reſiduum "was phlogiſticated air, and had the ſmell of the volatile "alkali."—Now here, evidently, the iron attracted the inflammable part of the air, and formed with it an impregnation of fire, ſomething ſimilar to the volatile alkali.

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* "There is an experiment mentioned by Dr. Higgins, which he ſays is in favour of Mr. Lavoiſier's ſyſtem; as indeed we muſt agree with him that it is directly contradictory to the ſyſtem of phlogiſton, with the belief that dephlogiſticated air is, in reality, dephlogiſticated. But if Doctor Higgins and others will pleaſe to ſee the truth, this dephlogiſticated nitrous air is formed of the nitrous acid and the phlogiſton of the copper; it will be a ſtrong proof in favour of the doctrine of phlogiſton. The Dr. ſays (page 550.)' "If tin be introduced into a neutral ſolution of the tin in the nitrous acid, it is calcined; a calx is thrown down, and the dephlogiſticated or imperfect nitrous air is produced.—Dephlogiſticated nitrous air, according to the phlogiſtians, contains no phlogiſton; then I aſk, What becomes of the phlogiſton of the newly-calcined metal? If tin contained phlogiſton, either inflammable air or nitrous air would be produced, or a portion of the diſſolved tin would be precipitated in its metallic ſtate; neither of which will take place, if the experiment be well conducted."—My *Chemical Eſſays, page* 9.

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* But that it is owing to the nitrous air's acid, ſeizing upon the phlogiſton of the metals and the liver of ſulphur, &c. appears clear from ſome experiments of Dr. Prieſtley,—who ſays, (vol. iii. p. 146.), "That liver of ſulphur alſo decompoſes nitrous air by ſeizing upon its acid, ſeems to be proved by the following experiments." . . . . So he goes on, and after that gives more deciſive proofs.—But I muſt refer my reader to the Doctor's volume.——And in vol. v, page 192, he ſays: There is a ſpecies of nitrous air, "in which a candle burns either quite naturally, or with an enlarged flame, and ſometimes alſo with a crackling noiſe, and vehemence, as if it was true dephlogiſticated and inflammable air. There is ſomething ſo exceedingly remarkable in this ſpecies of air, eſpecially its property of admitting a candle to burn in it, when it is ſtill as fatal to animal life as any ſpecies of air whatever, that I have not been able to refrain from attending to it."

The abſurdities of our *aerial* chemiſts are truly laughable. Mr. Davy allows, with all the other chemiſts, that it kill animals in four or five minutes; and yet *he*, and ſome other *wiſe* philoſophers, ſuppoſing their lungs different from other animals, (apes of Laputa excepted), can breathe it, making them caper like Merry Andrews. But how do they breathe it? Take an immenſe bladder-full of it, (pure),—and incloſe within it their wiſe heads, ſo that no atmoſpherical air can have acceſs, and their capering and kicking would ſoon be over: a few minutes would ſtop the ſcene of buffoonery. Mr. Pictet, *you* breathed it, but *you* certainly are not a true diſciple, or elſe you would not have told of the girl in York cutting the ſame capers upon breathing the air of the atmoſphere. The Reviewers were very angry with you for telling the truth to the gaping, credulous multitude. If you go on this way they will ſwallow no more whales. You muſt know that "CRAM" is the order of the day, even from the Royal Societies to the loweſt plebeans.

Doctor Higgins alſo ſays: "Dr. Prieſtley found that "nitrous air, expoſed to liver of ſulphur for a day, was "diminiſhed one-third of its bulk; a candle burned in "the remainder with an enlarged flame."

Now here the nitrous air took from the liver of ſulphur in one day, ſufficient phlogiſton to form this air. But then it is an air formed of the ſtrong nitrous acid, and a ſtrong ſaturation of phlogiſton; and therefore, as Drs. Prieſtley and Higgins ſay, "ſupports combuſtion, but "deſtroys animal life, and is condenſible in water."—And if my *good opponents* will examine the water, it will be found to be the nitrous acid, highly phlogiſticated.—Need we be ſurpriſed at this air's combuſtion in its aerial ſtate, when the nitrous acid burns in ſo many different ſtates in its condenſed ſtate, as in gunpowder—alſo with the volatile oils, &c. &c. Oh what a Herculean labour have I, to hammer common ſenſe into our *French aeriliſts!*

Doctor Prieſtley alſo procured this air, in the greateſt abundance, from the nitrous acids and ſpirit of wine, and alſo the nitrous acid and the oil of turpentine: ſee vol. ii. page 130. And in order to judge if an acid prevailed in this air, (which he called *dephlogiſticated air)*, he mixed it with alkaline air: they inſtantly formed a white cloud, making the nitrous ammoniac, as he ſuppoſed; only half of the air united with the alkaline air; the other half, upon applying a candle, made a conſiderable exploſion. Do theſe experiments require any comment, you *wiſe aerialiſts!*

Yet, my opponent, I will *deign* to hammer a little common ſenſe into thee, tho' there never was a ſchoolmaſter who has had ſo arduous and toilſome a taſk as myſelf. . . . . What thinkeſt thou of this air burning combuſtible bodies, "with *an enlarged flame?*" THAT certainly implies it is a richer air for combuſtion than even oxygen gas: this MY *common ſenſe* ſays: and it alſo ſays that oxygen cannot be an element, as oxygen gas is ſaid to be pure, being not united to any other body: and yet here is an air more active (or pure) for combuſtion, and yet, reader! *not reſpirable to common mortals.* The bodies which Doctor Prieſtley found to form theſe airs, were *zinc, iron,* and *tin*—the ſelf-ſame bodies which Mr. Cavendiſh found to produce hydrogen, with the other mineral acids. And ſtill to corroborate it further, I will deſire my reader to peruſe, with careful attention, Mr. Chevenix's experiments, beginning at page 30, which I have quoted. Now I think I may be allowed to ſay that the hydrogen is given out when a metal is calcined, and alſo received when a metal is reduced. Then Doctor Prieſtley's proceſs for procuring the greateſt quantity of this gaſeous oxyde of azote, was—putting iron into a ſolution of copper in the nitrous acid: (ſee vol. v. p. 200.) He ſays, "It is aſtoniſhing" the quantity of air that he procured in this proceſs, producing a jar-full every day for a fortnight, beſides much more that he never collected. Now in my letter to Sir Joſeph Banks, in the Gentleman's Magazine for December 1794, I ſhew that the procuring this air, (or what I call *highly phlogiſticated* *nitrous air)* is owing to the ſlow calcination of the iron; for as the nitrous acid leaves the copper to calcine the iron, it does it very ſlowly, as Mr. Chevenix obſerves; and therefore there is ſo little heat produced as that the acid gets its full ſaturation of the phlogiſton of the iron: for I ſhewed, that where it is produced from zinc and other metallic bodies, if there is a quick or ſlow calcination it forms either nitrous air, or this gaſeous oxyde of azote; or placing the mixture in a heated veſſel, or in ice, they will produce theſe airs accordingly, or ſimilar to what Mr. Chevenix did, if I added any nitrous acid to the ſolution of copper with iron placed in it. The freſh nitrous acid acted with rapidity upon the iron, and they then only produced nitrous air:—juſt ſimilar to what he obſerved upon the copper's quick reduction,—from the ſame cauſe, by adding the marine acid to the mixture.

Now, my candid reader, do ſeriouſly reflect upon the production of this gaſeous oxyde of azote; the nitrous acid, with phlogiſtic bodies producing it, viz. ſpirits of wine, the oil of turpentine, and the metals which produce hydrogen in the greateſt abundance, viz. zinc, tin, and iron—nay, ſulphur. Then canſt thou ſuppoſe it is any other body than the nitrous acid, highly phlogiſticated? Nay, it will ſometimes actually explode, as Doctor Prieſtley found; and in proportion as you check the efferveſcence, you will make it ſtronger; that is, more inflammable. What is oxygen gas, but an acid neutralized with fixed fire, with water for its aerial compoſition?—What is the gaſeous oxyde of azote, but a ſtronger acid, and neutralized with a ſtronger or more concentrated fixed fire, alſo with water for its aerial compoſition?——But it will not do for animal reſpiration, as the blood cannot decompound or receive its phlogiſton from it, they attracting one another ſo ſtrongly, and alſo its fire being too fixed to be eaſily decompounded by the animal organs. But it is a better air for combuſtion, as the heat produced makes the acid penetrate into the phlogiſton, and ſets its fixed fire looſe; and therefore it will often explode of itſelf. Need we be ſurpriſed at this, when the nitrous acid and oils will burn of themſelves; alſo the *nitrum flammans*. Why the atmoſphere is the beſt air for animal life, is owing to its being formed of a weaker acid, viz. the aerial acid,—and alſo of a weaker and more looſe concentration of fire more of an alkaline nature than a phlogiſtic one, and therefore more eaſily decompounded and let looſe by the animal's functions. But it is a worſe air for combuſtion than either the oxygen gas made in the elaboratory or this gaſeous oxyde of azote; they both burn with crackling, or partial exploſions. Canſt thou, my candid reader, demand ſtronger or clearer reaſoning? It is almoſt equal to demonſtration *.

This air will greatly elucidate what takes place in the Galvanic pile. It is clear that there is an acid in the Galvanic fluid; this acid attracts the phlogiſton of the metals, or the alkalies in the diſks, and then receives its water of compoſition, and ſo forms airs accordingly. If the pile is formed of the high phlogiſtic or calcineable metals, the air will be more inflammable; but if the leſs calcineable metals, (as platina, gold, &c.) the air will accordingly be leſs inflammable, or more of the nature of oxygen. This I will (I hope) indiſputably prove when I come upon Galvaniſm.

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* The nitrous acid and zinc produce this air, but when diluted much with water, produce nitrous air: (ſee Dr. Prieſtley, vol. iii.) Bodies produce air with the nitrous acid, according to the phlogiſton they contain; thus, biſmuth produces principally a nitrous vapour; ſilver, lead, &c. nitrous air; but zinc, this gaſeous oxyde of azote, (as it is abſurdly called). Alſo the production of theſe airs, depend upon the quick or ſlow effervefence of the quantity of moiſture with which the acid is diluted.—Doctor Prieſtley mentions an experiment, (vol. iii. page 201.) where the nitrous acid, being much diluted, only produced with zinc, AIR ſimilar to what biſmuth produced with the nitrous acid.

I would have my reader particularly to attend to this gaſeous oxyde of azote, as I am certain that it muſt lead to a full conviction of the truth of my theory. It will not effect the juice of turnſole, neither will it incorporate with alkaline air; and, inſtead of taſting acid as the nitrous air, it taſtes ſweet. (See Dr. Prieſtley, vol. vi. page 327.) Therefore the nitrous air it is made from loſes its acidity in the proceſs. But my "Thoughts on Air," which I publiſhed in 1785, clearly elucidates the ſubject; ſo that I thought it moſt extraordinary that it did not operate upon the public.—But I ſhall juſt here enlarge a little more upon it; and I ſhould hope that a little *common ſenſe* will be allowed in theſe *aerial* diſcuſſions: I want nothing more to produce conviction.

If zinc, or tin, be mixed with a diluted nitrous acid, firſt this gaſeous oxyde of azote is generated in great abundance; and after that, if the mixture is diſtilled, more of this air is produced, and then the pure oxygen gas. This experiment is eaſily performed, and I ſhould wiſh to have it carefully repeated: but if my reader does not chooſe the trouble of the experiment, and if he would chooſe to have ſome other authority than mine, I will give it him*.

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* Its taſting ſweet is directly ſimilar to the vegetable bodies; as malt, for inſtance, becomes ſo from the ſame cauſe, attracting the fixed fire

Therefore ſtill to enforce to thee, my reader, that black is not white, I ſhall quote an experiment from Dr. Prieſtley, (vol. iii. page 25.) = "Leſt this intervention of the inflammable nitrous air between the other kinds of air, and the pureſt of all, ſhould have been occaſioned by ſome accident, I carefully repeated the experiment, and found it in the ſame place a ſecond time. Having diſſolved a quantity of zinc in a quantity of one-fourth ſpirit of nitre and three-fourths water, diſtilling the ſolution to dryneſs, and receiving the produce in four portions, the firſt was only common air expelled from the phial, and not ſenſibly injured; the ſecond was *inflammable nitrous air*, (as we may call that kind of nitrous air in which a candle burns with an enlarged flame); the third was dephlogiſticated air, about four times as pure as common air; and the fourth was as pure as any that I have ever examined."

Dr. Prieſtley, in ſpeaking of this air, (vol. iii. p. 136), ſays: "I let down the zinc into the (nitrous) acid. Immediately upon this there was a moſt prodigious effervescence; but the veſſel into which it was made being very large, no part of the air eſcaped out of it;" and "a

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of the atmoſphere. Sugar, *for inſtance*, is *only the vegetable acid* highly neutralized, or ſaturated *with fixed fire, in thoſe hot countries where fire* ſo much abounds.

Dr. Prieſtley, in the *ſummary view of his principal facts*, vol. v. page 344, ſays, "This gaſeous *oxyde of azote is procured by the ſolution of* iron, with heat 133; *but in this caſe it burns more like* inflammable air, 134." Now clearly *from the iron, by the aid of heat ſaturizing it with* more phlogiſton. Why *zinc produces it in ſuch great abundance*, is owing to its containing *ſo much phlogiſton*. Dr. Gren, (vol. ii. p. 377), ſays, "But when melted *zinc is heated to redneſs in open veſſels, it takes* fire, and burns with a *very dazzling flame*."

candle burned in it with an enlarged flame, almoſt twice as large as its own proper flame—very vivid, and ſurrounded with another flame, of a blue colour." Now only conſider this experiment. In calcining the zinc, according to the French abſurdities, the nitrous acid ſhould have loſt its oxygen, and only nitrous air ſhould have been produced. But here is an air that ſupports flame with conſiderably more vigour than pure oxygen gas, and yet it is conſiderably lighter; therefore formed of purer materials for combuſtion.

But as this *aerial* fabric is ſo pertinaciouſly adhered to, it may be ſaid that this calcination is different from other metallic ones, and that ſome of the conjurer *Azote* may be concealed in the mixture. But that is directly contradicted by the quotation I have juſt made, for the mixture produced the pureſt oxygen gas alſo.——Good God! how hard has been my fate for four-and-twenty years, to have ſuch groſs abſurdities to combat! and myſelf, and my important truths, to be treated with ſuch unheard-of cruelty! I will pledge myſelf there is not an inſtance equal to it in the annals of literature.

When I was upon the exmuriatic acid, I hope I clearly proved that its qualities are owing to the acid being impregnated with a ſmall part of the manganeſe, or lead united to a quantity of fixed fire, which they would receive in the act of efferveſcence, and alſo in the act of diſtillation. Both the manganeſe and red lead receive a quantity of fixed fire, which they imbibe in their calcination; and this fixed fire is attracted by the marine acid, along with a part of the manganeſe and lead.

To corroborate this doctrine, there is another proceſs of making this oxmuriatic acid—which is by mixing the marine acid with the nitric acid; and they ſay that the marine acid attracts the oxygen from the nitric acid.— But to ſhew the error of this opinion, that it is *not* from attracting the oxygen from the nitric acid; but owing to the nitric acid poſſeſſing ſome fixed fire, and which fixed fire makes this acid; for all mineral acids are the ſame, and may be formed into each other. As in the oxygenated muriat of ſoda, upon the vitriolic acid being poured upon it, there immediately iſſues from it a ſtrong ſmell of the nitrous acid, it receiving in the proceſs the fixed fire neceſſary to form it, beſides many other proceſſes that might be named. But the ſtrongeſt *aqua regia* that can be made, is by impregnating the marine acid with the phlogiſticated nitrous acid, which has got an high impregnation of fixed fire, as the nitrous vapour. Now this nitrous vapour is united to fixed fire, as it is procured from the calcination of biſmuth, by the nitrous acid. (See Dr. Prieſtley, vol. iii. ſect. 18.) And alſo, as he ſays, will decompound atmoſpherical air, even quicker than nitrous air*. It ſhews how highly phlogiſticated it is, from its red colour. Dr. Prieſtley impregnated various bodies with this vapour, and alſo the acids. He ſays, (vol. iii. page 219.) " One of the moſt remarkable circumſtances attending this impregnation is, that whereas the common *aqua regia* is made in the beſt manner by mixing one-fourth of ſpirit of ſalt with three-fourths of ſpirits of nitre; this liquor, which is, in a manner, nothing but ſpirit of ſalt, (for I did not perceive that the bulk of it was ſenſibly increaſed by the proceſs), after having imbibed a little of the nitrous vapour, becomes poſſeſſed of all the properties of that *aqua regia*, which conſiſts chiefly of ſpirit of nitre, and in much greater perfection. For, while the common *aqua regia* will hardly diſſolve gold without the aſſistance of *heat*, this diſſolves it with as much rapidity as I have almoſt ever ſeen in any chymical ſolution whatever, when it is perfectly *cold*. The quantity of gold that this *aqua regia* is capable of diſſolving, in proportion to its bulk, I have never aſcertained; but it ſeems to exceed the common *aqua regia* very much in this reſpect."

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* He ſays, (vol. iii. page 192.) " I found, in the courſe of theſe experiments, that the power of this red vapour to phlogiſticate common air, was much greater, and acted much quicker, than I had imagined when I made the firſt obſervation of the kind. For after the former obſervations I filled another phial with the red vapour, and immediately afterwards opened it under water; when the water, ruſhing in, filled about half of it, and the remaining air was found completely phlogiſticated, not being in the leaſt affected by nitrous air."

Now, let me call upon the French theoriſts coolly and ſeriouſly to reflect upon theſe experiments. They ſuppoſe that in mixing the nitric acid with the marine acid, that the marine acid attracts the oxygen from the nitric acid, ſo as to form itſelf into the oxmuriatic acid. But here it appears, that the nitrous vapour which was procured by the calcination of biſmuth, therefore according to them, *deoxygenated*, that it ſhould produce a ſtronger *aqua regia* than the pure nitric acid, and that ſo ſmall a quantity of the vapour being required; ſo that it adds nothing to the bulk of the marine acid, and what a great quantity of the nitric acid is required! viz. three-fourths; and this phlogiſticated nitrous vapour making it a ſo much ſtronger *aqua regia*, juſt the direct *contrary* to the French theory. So that we ſhould ſay, it would *deoxygenate* the marine acid, inſtead of *oxygenate* it. They might with the ſame propriety ſay *BLACK is WHITE.*

Good Heavens! what a taſk have I, to have my time employed in detecting ſuch obvious, groſs, and glaring abſurdities Dr. Black ſays, This wiſe age of *aerial* chemiſtry, think themſelves wiſer than any others. So do all fools.

But let me argue upon theſe facts with a little *ſolid ſenſe*, (not *aerial)*. In forming this oxmuriatic acid, in all the proceſſes there is fixed fire required. Thus, in minium and mangaueſe forming it, both theſe bodies are formed by calcination, or being for a long time expoſed to the fire ſo as to fix a certain quantity of it, turning the white calx of lead *red*, and the mangaueſe *black*. Mr. Smithſon ſays, "Subjected to the action of the blowpipe on the coal, calamine became yellow the moment it was heated, but recovered its priſtine whiteneſs on being let cool.—This quality, of temporarily changing their colour by heat, is common to moſt, if not all, metallic oxides; the white growing yellow, the yellow red, the red black."

Alſo fire is further required when the lead and manganeſe are mixed with the marine acid, ſo that as the acid is diſtilled, it comes over highly impregnated with fixed fire. Alſo, in mixing the nitric acid and marine acid together, the marine acid attracts this fixed fire which the nitric acid contains. I have all along ſhewn that the nitric acid is formed in putrefaction, and thoſe proceſſes which contain a quantity of fixed fire. I likewiſe ſuppoſe that the electric fluid forms the nitrous acid from its fire. But the ſtrongeſt oxmuriatic acid is formed by this acid uniting to the fixed alkali a body formed of fixed fire; ſo that Mr. Chevenix calls it *hyper-oxmuriatic acid*. Alſo it is formed by the electric or Galvanic fluid uniting itſelf to the marine acid. (See Mr. Cruickſhank's experiments in Nicholſon's Journal.) And that this fluid contains fixed fire, I think I need not prove. But, Oh what hammering it requires to bring ſome people to their ſenſes!

This nitrous vapour is juſt as quickly imbibed by the vitriolic acid, phlogiſticating it ſo as to turn it blue.—— But I refer my reader to Dr. Prieſtley's 3d volume*.

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* That bodies receive fire by being evaporated or diſtilled, is clearly obſerved from every proceſs in chemiſtry. Dr. Prieſtley, in the ſummary view of his principal facts, ſays, "Heat deepens the colour of the nitrous acid. Its colour is univerſally owing either to phlogiſton or heat."—By being diſtilled it receives ſuch a quantity of fire as to form a little oxygen gas.——But the way to form the acids into oxygen gas, is to unite them with other bodies, earths, &c. and then, by having ſo ſtrong an attraction for the earths, they will not be ſeparated or evaporated from them till they have got ſo ſtrong a ſaturation of fire as to form either fixed air, azote, or oxygen gaſſes; but if they are united to the fixed alkalies as their attraction, for the acid is ſtronger, and alſo as the alkalies are formed of fixed fire, they form a greater proportion of oxygen gas. But the vegetable acid being a weaker acid, therefore when united to earths they produce airs of a leſs impregnation of fire, as principally azote, (page 79.) But when this acid is united to the fixed alkalies, and diſtilled, according to Lavoiſier, they form together ammonia. When fixed air is united to the calx of mercury, as when the marine acid is precipitated by the carbonated alkali, the corroſive ſublimate for inſtance, the fixed air is precipitated with the calx, and they form by heat the pureſt oxygen gas, and no fixed air, and the mercury is revived. (See Kirwan on Phlogiſton.)—But the fixed air, with calcareous earths, is ſet looſe as fixed air and azote, by fire. (See page 7.)—Alſo the phoſphoric acid has ſuch a great attraction for the earth of lead, and alſo for fire, that it is evaporated or aerialized as inflammable air. (See page 63.)—Alſo this is fully demonſtrated by the electric fluid. It is well known to contain a great quantity of fire. It will therefore reduce metals, and if paſſed through the phoſphoric acid, produce the ſame phoſphorous inflammable air. But if paſſed through the nitrous acid, it would only produce oxygen gas, as Dr. Prieſtley found. (See his Experiments *on paſſing the electric fluid through different bodies;* and

But I know that I muſt make my ground ſo clear, that there is no *buſh*, *fox-hole*, or *aſſaſſin*'s *den*, for my opponents to conceal themſelves in. They may ſay that this nitrous vapour is not phlogiſticated, or deoxygenated.—Let us ſee what Dr. Prieſtley ſays of this vapour, (vol. iii. page 221.)

" When ſpirit of ſalt is ſaturated with nitrous vapour,
" it yields air, in the ſame manner that water does in the
" ſame proceſs. This air I caught, and found it to be
" nitrous air, the ſame that it yielded by water."

Now here then, if this vapour is deoxygenated by the marine acid, by attracting its oxygen, water muſt *equally* deoxygenate it by attracting its oxygen. . . . . Oh *wiſeheads!* But does not this clearly tell you, *yea, and hammer it into you*, that it can only receive water from the water, and therefore the ſame from the marine acid, to form its water of compoſition. Thererfore, if one and one make two, it muſt diſſolve gold, from its fixed fire, or phlogiſticated nitrous vapour. And why theſe phlogiſticated vapours aid in calcining theſe ſtrong metallic bodies, is owing to their fixed fire in the act of calcina-

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alſo my Obſervations upon them in my Thoughts on Air.)——But can it be ſuppoſed, you *wiſe-heads* in theſe experiments of the Doctor's, that when he paſſed the electric fluid through the phoſphoric acid, that they ſhould produce this high phoſphoric inflammable air? and when through the nitrous and vitriolic acids, oxygen gas? I ſay, can theſe facts be reconciled either with his hypotheſis or Mr. Lovoiſier's? But with mine, nothing can ſhew a more beautiful and regular chain or concatenation of facts, impoſſible to be miſtaken by *terreſtrial* mortals, but eaſily by *aerial*. And I ſuppoſe that theſe clear and indiſputable facts would fully explain how the airs are generated by the Galvanic fluid, and not owing to that abſurdity of all abſurdities, the decompoſition of water; they might as well ſay, *the decompoſition of* TOM FOOLS.

tion, or during the great effervefcence induced, being ſet looſe as free fire, and ſo gives activity to the acid to penetrate the metal. This is clear; the operation of the gaſeous oxyde of azote, which is the higheſt aerial ſaturation of the nitrous acid with phlogiſton, its ſupporting combuſtion is clearly from this cauſe. Its own fixed fire being ſet looſe, ſets looſe the combuſtible bodies' fire.—This is alſo clearly the caſe with the oxmuriatic acid air producing combuſtion when bodies are placed in it;—(ſee Mr. Weſtrumb's experiments;) where various bodies are burned in it by mere expoſure, without fire being applied. This highly phlogiſticated acid penetrates or attacks the phlogiſton of bodies, and in that attack its own looſe phlogiſton is ſet free, giving activity to the induced action. It is from this it bleaches linen, &c.—It attacks the reſinous parts of the yarn, and deſtroys it. The ſame in vegetable colours. The colours of vegetables are owing to their high phlogiſtic parts repelling the rays of light ſo vividly as to ſtrike the *retina* with different impulſes, ſo as to produce the different colours.—Now, this highly phlogiſticated acid attacks this vegetable phlogiſton, partly ſetting looſe its phlogiſton as free fire, and partly uniting to it, ſo as to make it ſoluble in water. And this is the action of oxygen gas in ſupporting combuſtion; its acid holds its fire in a more free and looſe ſtate; ſo that when actual fire is placed in contact with it and a combuſtible body, the oxygen gas's fire is ſet looſe, and then it acts in ſetting looſe the more fixed fire of the combuſtible body; all of which is clearly and fully explained in my former works.

Now, reader, thou wilt have a key to Mr. Chevenix's Experiments, quoted page 33. He alſo ſays, "The fact of the production of a peculiar gas by the diſtillation of nitro muriatic acid upon platina, has been obſerved by Mr. Davy in his *Reſearches*. (But as hyper-oxygenated muriatic acid was not known at that time, he could not ſay the real nature of that gas.) Dr. Prieſtley alſo mentions a peculiar gas produced by diſtilling a ſolution of gold in *aqua regia*." Now clearly, my intelligent reader, thou mayeſt anticipate theſe explanations. Platina and gold poſſeſs little phlogiſton, and are with very great difficulty bereft of it. Seen in the Galvanic pile they are hard to calcine, and give out air more of the nature of oxygen gas than inflammable air. Therefore, when diſtilled in *aqua regia*, the phlogiſton the acid received from the metal was ſimilar to what the nitrous acid received from biſmuth, viz the nitrous vapour; therefore would act in producing the oxmuriatic acid, and alſo the hyper-oxygenized. Titanium would alſo act the ſame with the marine acid and pot-aſh, and clearly from this cauſe; for Mr. Chevenix is obliged to ſay, "I have attempted the ſame experiment with black oxyde of manganeſe, but could not ſucceed." Now here, reader, obſerve that a calx with a great quantity of oxygen (as they ſuppoſe) would not ſucceed; while different metals which poſſeſs no oxygen but phlogiſton, ſucceeded in forming hyper-oxygenized muriatic acid, and were themſelves oxygenated.

But if this oxmuriatic acid was formed by receiving an addition of oxygen, we ſhould certainly be able to form it by mixing them together; but Doctor Gren is obliged to ſay, "Oxygen and muriatic acid gas, mixed together, do not produce oxygenated muriatic acid,"—though he ſays, "they form whitiſh clouds," therefore undoubtedly they chemically unite. This experiment I performed many years ago, and the atmoſpherical air became perfectly injured; I have found it a good teſt, (the ſame as nitrous air) in meaſuring the purity of the atmoſphere, oxygen gas, or the gaſeous oxyde of azote; by mixing them with the oxmuriatic vapour, an efferveſcence ſucceeds. But inſtead of the elaſtic oxmuriatic acid becoming by this means hyper-oxygenized, it becomes the common marine acid, and the airs completely injured:—juſt the reverſe of what their *celebrated theory* teaches!—But in theſe experiments, care muſt be taken to uſe no more oxmuriatic vapour than is neceſſary for the pure airs to act upon, or to complete or ſaturate the efferveſcence. Juſt the ſame with nitrous air and pure air: if there is more nitrous air than is neceſſary, the overplus will remain entire; or if too much oxygen gas, its overplus will remain entire.

Whatever way we look at this French theory with the eye of reaſon and common ſenſe, abſurdity upon abſurdity appear. Dr. Gren, in ſpeaking of the gaſeous oxyde of azote, ſays—"During its production the baſis of ni-"trous gas is deprived of part of its oxygen, but not of "the whole quantity it contains." Now oxygen gas, which they ſay is *pure*, is conſiderably heavier than this nitrous air; and that nitrous air is in part the radical of the nitric acid, having oxygen taken from it, Dr. Gren ſays, (vol. i. page 276.) "Hence the radical of nitric acid "is capable of various degrees of oxygenation. The firſt "and loweſt degree forms the baſis of the gaſeous oxyde "of azote; the next conſtitutes the baſis of nitrous gas; "the third produces nitrous acid; and the fourth and "laſt generates the nitric acid." Now, if there is a particle of ſolid ſenſe amongſt our *aerial* philoſophers, how comes the air of the loweſt degree of oxygenation to burn bodies? As Dr. Gren ſays, (page 275.) "a lighted "candle burns in it with increaſed brightneſs,"—and which is a far lighter air than pure oxygen gas. Therefore, according to this reaſoning, this wonderful conjurer of all conjurers, *Oxygen*, muſt, with all its *hocus pocus* qualities be the principle of levity, as the higheſt ſaturation of it is a lighter air than oxygen gas. But then we are ſtill aground. Here is nitrous air will not allow bodies burning in it, which they ſay is formed from looſing its oxygen; yet by looſing more oxygen it becomes a lighter air, and then admits bodies burning in it with this *bright* flame! Where the *Diable* will this French theory lead us to at laſt? To *ridicule* and *contempt*.

Let us inveſtigate it another way. The nitric acid and eſſential oils produce combuſtion. But if the ſtrength of the acid is in the leaſt diminiſhed, no combuſtion takes place, but they form this gaſeous oxyde of azote.—— Now the French theoriſts ſay, that both theſe proceſſes are *the ſame;* they are both the carbon and hydrogen of the oil of turpentine, receiving the oxygen of the nitric acid. But in one caſe there is a moſt violent ſeparation of light and heat; and in the other comparatively none. Then let us examine the reſiduum after the proceſs; if that will help our *wonderful theoriſts*. According to them the nitric acid ſhould have lost all its oxygen, it forming with the eſſential oils carbon and hydrogen, fixed air and water, and its azote ſhould be left by itſelf. But we find that Dr. P. found this gaſeous oxyde of azote united in part to the nitrous acid;—for upon mixing them with alkaline air, he produced the nitrous ammoniac. Alſo, after firing the eſſential oils with the nitric acid, the reſiduum is a phlogiſticated acid and vapour. Now I call upon this *honourable combination*, if they can withſtand theſe abſurdities; and upon the *ſervum pecus*, if they can *ſwallow them*.

In the calcination of metals with the acids, water is required with the vitriolic and marine acids. They ſay, the water gives the oxygen to the calcination; but in the nitrous ſolution, not the water, but the acid gives the oxygen. Then why is the water alſo required in the nitrous ſolutions? Upon burning oxygen and hydrogen gaſſes, an immenſe quantity of light and heat are produced and ſet looſe. Then, in the vitriolic and marine ſolutions of the metals, they ſay this water is decompounded again, forming hydrogen gas. Then, from whence proceeds its light and heat, to form this gas? (for it had been diſſipated when the water was formed, in firing the oxygen and hydrogen gaſſes.) Nay, the oxygen that went to form the calx, must have carried with it its light and heat, as the calces give their light and heat to the marine acid when they oxygenate it;—as the calx of lead, &c. Alſo, different calces explode with combuſtible bodies. But as water is required in the nitrous ſolutions, does not this imply it acts the ſame part in both, to form the *aerial compoſition* of both the *inflammable* and *nitrous airs;* and alſo, along with the acids, to ſaturate the calces*.

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* That the water acts in aiding the ſolutions, appears evident—for when the acids have no water, heat will produce the calcination without it, and then the vitriolic acid air is formed. Heat juſt puts the vitriolic acid upon the ſame activity, in ſeizing upon the phlogiſton of the metal as the nitric acid which poſſeſſes fire.

When the metals are diſſolved in the different acids, either nitrous air is formed or hydrogen gas; but when the metals are diſſolved in oxmuriatic acid, there is no gas formed. The French theory ſays, This ſolution is owing to the metal receiving the oxygen of the acid.—But from whence comes the inflammation obſerved in this ſolution?—Dr. Gren ſays, (vol. i. page 308.) "A lighted wax candle continues to burn in the elaſtic oxygenated muriatic acid, though with a diminiſhed and duller flame. Phoſphorus, charcoal, cinnabar, grey antimonial ore, antimony, biſmuth, zinc, and ſeveral other combuſtible bodies reduced to powder, are even ſpontaneouſly inflamed when thrown into this elaſtic and warm-

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If I add only as much nitric acid and water to a metal as to calcine it, they ſay that the nitric acid is decompounded. But Mr. Metherie ſays, (page 146.) "Though nitrous air is obtained from a ſolution of "mercury in nitrous acid, *almoſt all the acid* is formed in the ſolution."—And Dr. Fordyce ſays, "In the ſolution of zinc in the vitriolic acid and "water," he "found *almoſt all the acid* in the ſolution."——Now they ſay, that in this ſolution the water is decompounded. But Dr. Fordyce found a ſmall part of the acid had diſappeared, it evidently giving the hydrogen that hepatic ſmell; juſt the ſame as Mr. Metherie found, a ſmall part of the nitric acid had diſappeared in the ſolution. Then can we *ſuppoſe* that the one ſolution is from a decompoſition of the nitric acid, as by adding no more acid than would calcine the mercury, no part of it ſhould be found, it all being decompounded. But it was ALMOST ALL FOUND: therefore, undoubtedly, both the nitrous and the vitriolic ſolutions are the ſame; for they both require water; one part of the acid and water entering the nitrous and hydrogen gaſſes, and the other part entering the calces.

The oxmuriatic acid, and an alkaline ſalt, form the oxmuriatic ſalt; and Van Mons almoſt formed a ſalt with this acid and the volatile alkali, which, according to their theory, ought not to have been done. The oxygen ſhould have united to the hydrogen of the ammonia, and formed water.

ed acid. Theſe inflammable ſubſtances deprive it of a part of its oxygen, and convert it into the imperfect or muriatic acid."—Alſo he ſays in the ſame volume, (page 308.) "Hydrogen gas affords, with oxygenated muriatic acid, when in the elastic state, a mixture that may be ſet on fire. The product of the combustion is liquid muriatic acid."

And this oxygen the acid received from lead, and the metals receiving oxygen from the atmoſphere is an act of combuſtion; therefore when the oxygen entered, the lead muſt have parted with its air, fire and light. Then from whence comes the fire and light in the combuſtion of theſe bodies?

Therefore, clearly, the reaſon why the ſolution of metals give out fire and light in the oxmuriatic acid, is owing to the combuſtion of their phlogiston: for the hydrogen gas, and even nitrous air, with the elastic oxygenated muriatic acid, are ſet on fire: (ſee Gren.) But, according to the French theory, why is zinc ſet on fire in this acid; and produces, with the nitrous and marine acids, no combustion but nitrous and inflammable airs—as the proceſſes are the ſame, according to them; the zinc receiving oxygen? This I will declare, in the face of Reaſon and Common-ſenſe, (and they that diſpute it poſſeſs neither), That in the one caſe there is a combuſtion, (that is, a ſeparation of light and fire); and in the other caſe, this light and fire is not conſumed, but is exhibited in the form of hydrogen gas, when the marine acid is employed in the ſolution of the zinc, and the gaſeous oxyde of azote, when the nitric acid is employed.—For, *ye devoted French tools and ſycophants!* If the ſolution of the zinc in the nitrous acid is conducted ſlowly, it will produce an air, that will, if fire be put to it, explode of itſelf, ſhewing this fire and light. And if the ſtrength of the oxmuriatic acid is damped by being much diluted with water, or the common marine acid added to it, they will form (with metals) the marine acid air—an air ſimilar to the nitrous air, or the vitriolic acid air.

There is an acid of ſome peculiar qualities which they call *the Pruſſian acid*: it is made from animal ſubſtances, bones, blood, &c. The acid which the animal kingdom produces ſeems to be the *phoſphoric*, the vegetable kingdom the *vegetable acid*. But by a peculiar preparation, by calcining the animal bodies with the fixed alkali, inſtead of producing the phoſphoric acid, this Pruſſian acid is produced. Doctor Gren ſays, (vol. ii. page 30.) "By diſtilling the Pruſſiat of pot-aſh, the Pruſſic acid is deſtroyed, and the products of the proceſs are carbonated hydrogen gas, and carbonat of ammoniac. The reſidue conſiſts of vegetable alkali and iron, and alſo of phoſphoric acid. Hence the radical of Pruſſic acid is compoſed of carbon, azote, hydrogen, and phoſphorus."

He further ſays,—"If upon Pruſſiat of pot-aſh, contained in a tubulated retort, connected with the pneumatic quickſilver apparatus, ſome dilute ſulphuric acid be poured, and heat applied, the Pruſſic acid diſtils over; but in the form of elaſtic fluidity. It then conſtitutes a peculiar kind of air, viz. the *Pruſſic acid gas*, which is inflammable, and yields, after combuſtion in oxygen gas, carbonic acid, water, and azotic gas; is poſſeſſed of a pungent ſmell and taſte; is abſorbed by cold water, but flies off again from it in warmth and on expoſure to air.'

Now I think we muſt concede to the opinion, t the phoſphoric acid is changed by the calcination

Pruſſian acid; for the calcination of theſe bodies, without the alkali, is well known to produce the phoſphoric acid, though even in this caſe a very ſmall part of the phoſphoric acid is changed into the Pruſſic acid. But if the ſame proceſs be applied to the Pruſſiat of pot-aſh, viz. the diluted vitriolic acid, this Pruſſic acid gas is procured; therefore the phoſphoric acid of the animal bodies is turned into the Pruſſic acid. This ſeems to be brought about by the fixed fire of the animal bodies; alſo the fixed fire of the alkalies, and the fire employed in the calcination. And it alſo appears, that when this Pruſſic acid gas is fired with oxygen gas, inſtead of producing the phoſphoric acid, they produce the carbonic acid, water, and azotic gas. Therefore I think this muſt clearly teach us, that all the acids are the ſame, having the ſame origin, but they are changed into different acids, and that principally by fire or phlogiſton; and alſo that azote has its origin from an acid phlogiſticated or neutralized; and that the aerial acid (viz. the carbonic acid) is from the different acids aerialized.—But when I ſhall treat particularly of acids, I ſhall (I hope) make this plain.

I might go on for a long time ſhewing equal abſurdities that this *French* theory is guilty of; but I am *tired*—for if what I have already ſaid is not ſufficient to produce conviction in my reader, neither his *head* or *heart* are capable of conviction—I give him up to the liſt of INCURABLES of *French maniacs*.—But why ſo many deſerters from the ſcientific honour of your country, in favour of France? Shall a few men, great in pecuniary influence, make you apoſtates, becauſe they think their experiments have been aiding in eſtabliſhing this abſurd and ridiculous *French ſtructure?* Engliſhmen! aſſume your dignity. You are high in the eſtimation of the world, for honour, liberality, and knowledge; a candid and impartial inveſtigation is all I want: for even though France is ſo great an enemy and rival to England, I would not rob her of her juſt merit, much leſs would I endeavour to deprive her of a *fair and honourable trial.* The good and honour of true ſcience is all I want—the candid inveſtigation of TRUTH! I ſtand alone, inſulated in an obſcure corner of the kingdom; but, I hope, *firm as a rock*, upon the pedeſtal of Truth, and therefore not afraid to engage with the combination, with all its united force, knowing *their* theory to be a *baſeleſs* aerial French ſtructure, and now only ſupported by *baſe* artifice.

But it *muſt* and *ſhall* fall: then beware upon whoſe heads it tumbles; for it is a huge enormous pile, built by *Ignorance*, propped by *Craft*, and ſupported by *Injuſtice.*

I ſee Dr. Thomſon of Edinburgh, whoſe great labour is to clear the French theory of combuſtion from *ſome few* of the abſurdities I have ſhewn it is full of. In my theory I prove that pure air is the *agent* to combuſtion.—He (to appear not to have taken the idea from me) calls it the *ſupporter:* a different name, but the ſame idea.—But he, to differ from my theory, (for he ſteers partly *by* it) ſays that oxygen gas only contains heat; the combuſtible body, light:—an idea taken from Dr. Gren. But that *light* and *heat* are the ſame body, I have inconteſtibly proved.——(See my experiments on Volta's pile, and my new ſyſtem of fire and planetary life.)——But *light* ſeems to be a better body for forming oxygen gas than *heat*—ſee its action on oxmuriatic acid, nitrous acid, &c. And *heat* is well known to revive the calx of metals—*ſee* its action on the calces of mercury, even when it paſſes through the blackeſt bodies in getting acceſs to the calx*. So that a great defender of the French theory (Count Rumford) thought that *light* acted by its *heat* in reviving the calces of metals.

Dr. Thomſon ſets out with contradicting the French chemiſts, that bodies whenever they imbibe oxygen, combuſtion is produced. He ſays, "Hence we ſee that com-"buſtion ought not to be confounded with the combina-"tion of a body with oxygen, as is done by the French "chemiſts." He then elucidates this doctrine as follows.

He ſays that when oxygen gas is imbibed; there is not always a combuſtion. He mentions the calces of metals, viz. the calces of lead. Their firſt doſe, he ſays, produces combuſtion; but the doſe of oxygen, that makes the grey calx and letharge turn to the brown and red, he ſays enters with their caloric, producing no combuſtion. Now that the grey calx of lead and letharge contains no oxygen gas but fixed air, and that this fixed air is formed into oxygen by heat, I hope I have ſufficiently ſhewn. But theſe French theoriſts diſpute the commoneſt facts. But nothing can be ſo abſurd as the whole paper. The vitriolic acid is made a combuſtible. I am ſurpriſed that men ſhould ſo far leave common ſenſe behind them—But all is now *aerial!* All ſolid bodies, they ſay, are made of *airs;* ſo I ſay, all ſolid reaſonings are now *aerial:* The ſtrongeſt combuſtion that we know of is the burning of ſulphur; and even in their *aerial theory* the combuſtion of ſulphur imbibing the oxygen gas, was one of the greateſt and firſt experiments that made Lavoiſier adopt his theory of combuſtion: and therefore, certainly, the vitriolic produced by their combuſtion muſt claim the rank of a product. No, it is a combuſtible, Dr. Thomſon ſays; and, ſtill more wonderful! nitrous air (he ſays) is a *ſupporter*. But nitrous air is the only aerial body that will unite with oxygen gas with the greateſt avidity, in any temperature producing the nitrous acid. Here then the oxygen gas muſt be the *combuſtible*, and the nitrous air the *ſupporter*. Wonderful! Azote he places at the head of the column of INCOMBUSTIBLES. But Mr. Gotlin ſays that phoſphorus burns in it with the greateſt brilliancy. But why am I debaſing my pen by expoſing ſuch abſurdities? I can only ſay, the French theory is *groſsly* abſurd, and Dr. Thomſon is a *groſs* ſupporter, and their combuſtion together will produce no *light* or *heat*, but the *groſſeſt* darkneſs.

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* Why *light* is a better body than *heat* for reviving metals, is owing to its ſuperior impulſe, and penetrates the metals accordingly.

He ſays that the facility of combuſtion is not proportioned to the attraction of oxygen—which, if Lavoiſier's theory was true, it certainly ought to be. He names ſome inſtances of phoſphorus and charcoal; phoſphorus, burning in a low temperature, and charcoal requiring a high one; yet, according to his French theory, he ſays, "Charcoal takes oxygen from phoſphorus." Thus, in the vitriolic and phoſphoric acids, forming phoſphorus and ſulphur with charcoal: (but charcoal is ſuppoſed by them not to be a pure baſe, diamond being ſuppoſed to be the pure baſe; but phoſphorus they ſuppoſe a pure baſe.) Yet it is not by taking oxygen from the acid, otherwiſe an immenſe quantity of fixed air ought to be formed in theſe proceſſes. But there is *none* formed—there is only the vitriolic acid air. If *any*, it is only what charcoal is known to contain.——He accounts for this by ſaying, that "it is probably owing to the weaker "affinity by which their particles are united: for the "coheſion of heterogenous particles, when oxygen con-"ſtitutes a part of them, is uſually weaker than the co-"heſion of homogenous particles." Yet, unfortunately, the very example he gives, is the reverſe of this:—for phoſphorus (he ſays) is robbed by charcoal of its oxygen; and, after all, he acknowledges that "nitrous gas and the "gazeous oxyde of azote, conſtitute exceptions to this "rule." I am ſurpriſed that any one can have the effrontery to name Mr. Cavendiſh's experiments of azote and oxygen gas, being formed into the nitrous acid by the electric ſpark, after what I have ſaid in the Experiments on the Galvanic Pile.

Inſtead of coinciding and according with the known and eſtabliſhed facts of chemiſtry, taught by our forefathers, and which have been regularly handed down to us,—which all have agreed upon, ſanctioned by common ſenſe and reaſon,—theſe wild theoriſts (to eſtabliſh their abſurdities) controvert. Thus, the acids, the purer they are, they act upon bodies, metals, &c. If you form a nitrous ſolution of tin, by adding iron to the ſolution, it leaves the tin gently to act upon the iron—nothing like the activity of the pure acid acting upon iron. I might mention a thouſand inſtances; but, according to Doctor Thomſon's *forced* principles, it ought to be reverſed.—Thus, the nitric acid united to azote, bodies burn in it more rapidly than in pure air; alſo phoſphorus burns in atmoſpheric air, in the common temperature, but not in oxygen gas. Alſo, ſtill more extraordinary! *Charcoal*, which they ſay is a body united to ſome oxygen, burns in a weaker temperature than *diamonds*, which are *pure* carbon. But, to adhere to my firſt example, for to ſuch groſs abſurd opinions I will not deign to fetch any more. It would affront common ſenſe. — By the acid being already united to a quantity of tin, it would in conſequence act with leſs activity upon the freſh iron or tin — would it not, ye blunderers! ye deſtroyers of common ſenſe!! But how rationally does my theory account for all theſe phenomena! Why the atmoſpherical air acts upon the phoſphorus in a lower temperature than oxygen gas, is owing to its being an air, with its acid united to a looſer ſaturation of fixed fire, and, in conſequence, eaſier ſet looſe. That the nitric and oxmuriatic acid's producing the combuſtion of high plogiſtic bodies, is owing to their high attraction for phlogiſton, &c. And as to diamonds being charcoal, they are no more charcoal than your brains are charcoal, Dr. Thomſon.—But how hard is my fate, to waſte my time upon ſuch abſurdities, to flog our *aerial* chemiſts into a little common ſenſe *!

But even Dr. Thomſon is obliged to contradict the obvious opinion of Cavendiſh and others, that the following experiment of Mr. Cavendiſh's is a burning proceſs; for he ſays, in the burning of hydrogen and oxygen gas, the nitric acid formed is not from combuſtion. In ſhort, they turn, twiſt, and twine poor common ſenſe, into all forms and ſhapes.

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* It appears that water is neceſſary to the action of the acids upon the metals; then, FOR WHAT REASON? It cannot be from the water aiding the decompoſition of the nitric acid, for inſtance; as, when it is added to the nitric acid, in acting upon the carbon and hydrogen gaſſes of the eſſential oils, it there retards its operation, hindering the combuſtion. Then why is it neceſſary in metallic ſolutions? Clearly, from the reaſons which I have given, (according to my theory.)

In metals, being calcined by one of the mineral acids and water, viz. the vitriolic, they ſay that water is the calcining body, being decompounded; but upon another mineral acid, (viz. the nitrous acid) and water calcining metals, they ſay it is not the water but the acid is the calcining body. And Dr. Thomſon, in this profound explanation of Lavoiſier's theory, ſays, " But in this caſe a new ſupporter is always evolved, viz. nitrous gas; as it happens a new combuſtible is uſually formed, and not unfrequently the product is converted into a partial ſupporter." Here then, in this calcination, inſtead of having the hydrogen gas ſo conſiderable a combuſtible body, we have nitrous gas a ſupporter. What comes then of the light? Upon nitrous gas and oxygen gas being united, no light comes from them. But then he ſays, ammonia is formed. I deny that upon metals being calcined by the nitrous acid, any ammonia is formed. The vitriolic acid and water calcining metals, is a better proceſs for forming it. But he contradicts it himſelf: for he ſays, " The product is converted into a partial ſupporter."—How then could it contain ammonia? The calx of iron formed by calcining in the atmoſphere, when diſtilled, forms ammonia, as Mr. Kerr obſerves; but I never knew that the calx of mercury, formed by the nitrous acid, when diſtilled would form ammonia. Indeed he groſsly contradicts himſelf; for he ſays, " Oxygen gas may be procured from nitric acid and oxmuriatic acid, two ſupporters; and from ſeveral of the partial ſupporters, as the black oxyde of mangneſe, the red oxydes of lead and of mercury. The action of heat is always neceſſary; but the proceſs is very different from combuſtion."——Where then is the combuſtible oxyde of mercury, as it gives out oxygen gas by heat? Theſe ſuperficial reaſoners daſh blindly forward, without ever reflecting it is truly laughable. I ſee this ſame philoſopher is now *puffed* as the great champion to reconcile the abſurdities of the French theory*.

That fixed air may be formed by the rays of the ſun into pure air, *I have proved in the moſt unexceptionable manner*, (and I call upon all the French theoriſts to contradict me, if they can) it is owing to the fire of the ſun neutralizing the aerial acid into pure air, becoming by that means fixed fire, and the water aiding the proceſs by becoming the water of compoſition. For if the *pureſt* diſtilled water be impregnated with fixed air and expoſed to the rays of the ſun, the pureſt oxygen gas will be formed. Why extraneous bodies aid this proceſs, is by their ſtopping the rays of light from paſſing through the tranſparent water. For, as Count Rumford indiſputably proved, that threads of ſilk, however repeatedly employed, and however repeatedly waſhed, would equally anſwer the proceſs,—therefore could not, *to the moſt prejudiced French theoriſt*, act by attracting charcoal from it. —Nay, Dr. Woodhouſe found that powdered charcoal equally anſwered, producing the pureſt oxygen gas—nay, any extraneous body. Why the charcoal did not produce ſo much oxygen gas, was, that it abſorbed the rays of light too much, and hindered them from penetrating into the body of the water.

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* Our chemiſts who are ſuppoſed to be the moſt eminent, have long been chaſed off the field by my works. But they now puſh forward every daring pretender, and a little time ſends them ſneaking off with the reſt. I have had many different ſets to combat with.

The phenomena of iron and ſulphur *burning*, were not firſt made in detail by the Dutch chemiſts in 1793, but by me in 1788; alſo a letter I publiſhed in the Gentleman's Magazine and my other publications. As to phoſphorus and the metals, and alſo barytes producing the ſame phenomena, I mentioned in my laſt letter to Mr. Cavendiſh, publiſhed in 1798. Theſe facts Doctor Thomſon names as originating with himſelf; and *all this is honourable*, not even to ſay ſuch a man as me exiſts. The *explanation* of all *theſe* phenomena may be ſeen in my Letter;—the action of heat upon phoſphoric bodies is fully demonſtrated in my laſt experiments upon the Galvanic pile, where I offer ſome strictures upon Herſchell's late papers upon *light* and *heat*. But, to be ſure, Doctor Thomſon, you did not ſteal any of your ideas from me in that publication;—no, nor will he acknowledge that he did not ſteal from me the following explanation, viz. "Now as pot-aſh is deprived of its carbonic acid by lime, "it is obvious that the proceſs muſt be a double decom-"poſition; the baſe of the lime combines with carbonic "acid, while its light combines with the pot-aſh."—— Which explanation may be found in the Gentleman's Magazine, 1792, where I controverted Dr. Black's doctrine of the phenomena of lime acting upon pot-aſh*.

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* I ſay, "Lime and cauſtic alkaline ſalts have loſt their fixed air, or aerial acid and water, and in their ſtead have united to fire. If lime, for inſtance, is mixed with the fixed alkaline ſalt, the alkali having a ſtronger attraction for the fire of the lime than for its own fixed air, will attract the fire of the lime, which has a leſs attraction for it than the alkali has; conſequently, the fire will leave the lime, and be attracted by the alkali This cannot be from the lime having a ſtronger attraction for the fixed air, or aerial acid, than the alkali, as alkalies attract acids more ſtrongly than calcareous earths do.

As Mr. Lavoiſier's theory of chemiſtry was founded on the combuſtion of bodies, (after my publication in 1781), I ſhewed the extreme abſurdity of the reigning theories, in ſuppoſing phlogiſton to be an element; I proved it to be fixed fire. Mr. Lavoiſier, upon this, began to exerciſe his ingenuity in dealing out a new theory, which has been generally received, particularly by our Engliſh theoriſts, who were determined to receive any

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"In conſequence of this miſtake, Dr. Black is wrong and contradictory in his table of attractions. He ſays,

ACIDS:	FIXED AIR:
Fixed alkali,	Calcareous earth,
Calcareous earth,	Fixed alkali,
Volatile alkali,	Magneſia,
Magneſia.	Volatile alkali.

"But he ought to have placed the different bodies under the head of fixed air, the ſame as under the acids; for the change of affinity is owing to the fire which the calcareous earths and the magneſia, in the ſtate of lime, poſſeſſed,—and which Dr. Black does not ſo much as acknowledge it does poſſeſs, not having a chemical knowledge of fire, or its affinity.

"But a ſtill ſtronger proof that alkaline ſalts have a more powerful attraction for fire than for fixed air, is this: Expoſe the dry cauſtic alkaline ſalt, or lime, to dry fixed air, and they will not attract it, nor part with their fixed fire; but add fixed air and water to the cauſtic ſalt, or lime, both acting together, their joint attraction will expel the fire, tho' neither can do it by themſelves."——This doctrine is ſtill more forcibly and fully explained in my "Obſervations on the Galvanic Pile."

But juſt to ſhew how I have been ſtolen from, ſee what Dr. Prieſtley has done, that hoary and arch-plagiariſt, who has been for theſe twenty years pilfering from me, and endeavouring to twiſt them to ſome fooliſh hypotheſis, not daring *directly* to adopt mine. In ſhort, my writings have given birth to *almoſt* all the philoſophical hypotheſes adopted for theſe twenty years, not directly adopting mine, but to build up ſome abſurd theory; there it is very ſtrikingly ſeen how they have been ſtealing from my "Theory of Fire and Planetary Life."—But they ſhall one day be ſeverely called over the coals. For this publication only adverts to my chemical writings.

one in oppoſition to mine. Therefore, as the theory of combuſtion is called Mr. Lavoiſier's theory of chemiſtry, I ſhall examine more cloſely Doctor Thomſon remarks; and ſhew how rationally my ſyſtem accounts for all the facts he mentions*.

Now, it appears ſingular that theſe oxydes ſhould poſſeſs oxygen in two ſuch different ſtates, the one being bereſt of its caloric and the other poſſeſſing it, as they are formed by the ſame proceſſes, by combuſtion; &c. nay, more ſtriking than that, is, the red and brown oxydes of lead, for inſtance, are produced by a far longer proceſs of burning, viz. 48 hours, as I have ſhewn, (ſee p. 12). Therefore one would have ſuppoſed, according to my old friend and reaſoner, *Mr. Common Senſe*, that the oxygen gas would have been more perfectly burnt, or robbed of its caloric, (to ſay nothing of Mr. Lavoiſier's general hypotheſis that gaſes are formed from caloric, and when they are attracted by ſolid bodies they loſe their caloric.)

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* He ſays, "The following oxydes are products of combuſtion:

1. Black oxyde of iron.
2. White oxyde of zinc.
3. White oxyde of lead.
4. Yellow oxyde of tin.
5. White oxyde of antimony.
6. White oxyde of arſenic.
7. White oxyde of manganeſe.
8. Oxyde of copper.
9. Oxyde of cobalt.
10. Oxyde of nickel.
11. Oxyde of biſmuth.
12. Purple oxyde of gold.
13. Yellow oxyde of ſilver.
14. Black oxyde of mercury.

The following oxydes, on the other hand, are partial ſupporters of combuſtion:

1. Red oxyde of iron.
2. Yellow oxyde of gold.
3. White oxyde of ſilver.
4. Red oxyde of manganeſe.
5. Arſenic acid.
6. Red and brown oxydes of lead.
7. Black oxyde of manganeſe.
8. Acidulous oxyde of antimony.
9. White oxyde of tin.

—But let us examine the oxydes of iron made by ſteam They muſt poſſeſs conſiderably more oxygen than the oxydes of iron formed any other way, as they are ſo much heavier; yet they will not act as ſupporters. And why ſhould they not? What hinders the carbon and hydrogen of bodies to attract their *extra* oxygen, and burn and produce a combuſtion of bodies, according to Dr. Thomſon's principles, formed of *light* and NO *heat*,—which would be a curious phenomenon in nature! But as we ſhould have a combuſtion without heat; ſo, in another caſe, we ſhould have a combuſtion without light, as ſome of theſe oxydes are only combuſtible in oxmuriatic gas, for they are perfect oxydes*.

But how comes oxmuriatic gas and the nitric acid to calcine gold and ſilver, when they cannot be calcined by combuſtion? How comes their oxygen, when united to its radicals, (viz.) azote, marine acid, &c. act upon the gold, ſilver, &c. when the pure oxygen gas cannot, which is not united to any body? It muſt appear ſingular, that when a body is attracted by another body (as azote), which it is ſuppoſed to have a ſtrong attraction for, (as Mr. Cavendiſh formed them he ſuppoſed by combuſtion) it can part with its oxygen in any temperature to thoſe metals, while the oxygen gas cannot be united when by itſelf, in the greateſt heat. Is there no abſurdity in this, I aſk? But let us try theſe acids by other proceſſes.—— Can you theoriſts breathe theſe bodies? for it appears that they part with their oxygen more readily than when free. In making the oxmuriatic acid, it is ſo noxious that the men cannot ſtand its vapours.——Can you, *ye wiſeacres!* ſuppoſe the calcination of metals by acids, is by the acids parting with their ſuppoſed oxygen to the metals? Can your lungs attract it from them? Apply either membranes, blood, or fat to the oxmuriatic acid, and it will burn them immediately: the nitric acid will do the ſame with the volatile oils. And this, they ſay, is the ſame proceſs as is performed on the lungs by reſpiration. . . . Go, *ye SOLOMONs!* and learn common ſenſe.——No, they act by having ſuch an attraction for phlogiſton, that they ruſh upon it with that activity as to ſet its fixed fire looſe as free fire.

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* But why the oxmuriatic gas forms a combuſtion in theſe caſes, is owing to its being formed of the marine acid, and earths united to a conſiderable quantity of fire; ſo that when theſe oxydes attract its acid, both their fire is ſet looſe.

But as to the oxydes which produce combuſtion with bodies, it is thoſe only which have got their fixed air turned into oxygen gas by the fire: (ſee page 12.) And alſo the oxydes of gold, ſilver, &c. why they will not burn with oxygen gas is, that they attract their fixed fire ſo ſtrongly, that common combuſtion wilt not ſet it looſe, and this aids their fulminating with combuſtible bodies, by attracting their fixed fire ſo ſtrongly, like the acids. That they attract ſo forcibly their fixed fire, is proved (as the immortal SCHEELE found) from the ſun reducing them by juſt throwing its rays upon them*.

But I ſee, in page 18, he ſays, that ſupporters part with their oxygen to combuſtibles without producing either light or heat. Now, as he ſuppoſes their oxygen to poſſeſs no *heat*, why ſhould not *light* be ſet free from the combuſtible? For if iron, for inſtance, poſſeſſes light, then it muſt appear ſingular, that when it is calcined by water, its hydrogen ſhould receive this light. But when, by the nitric acid, the nitrous air ſhould not receive this light, as he ſays it is a ſupporter — therefore muſt only poſſeſs heat; then what became of the light, and from whence came the heat? This is truly *hocus pocus* work of the Doctor's. I will relate a ſtory to him I think applicable.—— A decrepid old man was ſtepping over the kennel, and unfortunately he fell into it, *(as this French theory I hope has done)*. He bawled out luſtily, "GOD mend me!" A boy paſſing by, ſaid, *God mend* thee! *It would be far eaſier to* make *a new one than* mend *thee.*

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* For the Doctor ſays, "When ſulphate of iron is dropt into a ſolu- "tion of muriate of gold, nitrate of ſilver, or nitrate of mercury,—the "gold, ſilver, and mercury are precipitated in the metallic ſtate."—— Now here it alſo appears, that the fire the oxmuriatic poſſeſſes is capable of giving thoſe metals that are ſo eaſily reducible, their fixed fire again.

Doctor Thomſon is obliged to ſay, "But no ſubſtance, except a combuſtible, is capable of ſeparating hydrogen gas from water, by combining with its oxygen." But certainly this ſavours ſtrongly of the hydrogen coming from the combuſtible body; for, clearly, if it was from the water, we ſhould be able to decompound it by *ſome other body than a combuſtible one.*

The ſimplicity and beauty of my theory muſt ſtrike every man open to candour, reaſon, and common ſenſe; far different from the forced, abſurd CONCEITS of the French theory. Mr. Smithſon ſays, that "fire changes "the metallic oxydes,—the white growing yellow, the "yellow red, the red black." (See page 121.)—Juſt the ſame changes which the ſun makes upon fruit, which every man muſt have obſerved. Therefore, as fire produces theſe changes upon oxydes; when it is permanent or fixed, they muſt retain theſe colours or changes. But our bright *aerial* French philoſophy ſays, *Theſe changes* *are made by oxygen, the ſouring principle.* But then, you unfortunate philoſophers! are the acids red? Nay, they become, from yellow, as phoſphorus and ſulphur pellucid, from receiving this ſouring conjurer—juſt the reverſe! Indeed every department in nature gives the lie direct to their *aerial*, unſubſtantial, and vague theories. But they are unſubſtantial as the wind; and ſhall, "*like the baſeleſs fabric of a viſion*," vaniſh into "airy nothing," and "*leave not a wreck behind!*"—But it would be happy for its unprincipled partizans if their conduct could alſo vaniſh into "airy nothing," without *leaving* a *ſtain behind*, and which ages will not waſh away!

If Dr. Thomſon will only reflect a little . . . . But the misfortune is, that all men, now *daſhing* into philoſophy, know that they ſtand upon popular ground by defending this French theory: therefore, the little intellects they have, they never exerciſe them, to queſtion the validity of their opinions, or how far common ſenſe authorizes them; but indeed the more they differ from common ſenſe, the better; it is below an *aerial* philoſopher or chemiſt to be regulated by ſo *common* a thing: it therefore is very *uncommon* with our *new* philoſophers.—This conjurer, OXYGEN, is ſo twiſted and twined, and forced to explain every phenomenon; ſometimes in entering a body it parts with its dear caloric, and ſometimes they allow them to enter together. But I ſhall juſt confine Dr. Thomſon to the example I have given of the action of the nitric acid upon zinc—I might bring fifty more, but it is unneceſſary; in ſhort, it is not of conſequence enough for any more. Now this acid produces combuſtion in gunpowder, and with the eſſential oils the moſt active and violent combuſtion: therefore, when it parts with its oxygen, heat is produced; and therefore it muſt have been politely received by its baſe, **Azote**, with its dear paramour, **Caloric**. And when the marine acid is mixed with the nitric acid, in order to make the oxmuriatic acid, it receives a part of the oxygen of the nitric acid; and this oxygen which it receives is ſo very active in combuſtion, as we have ſhewn. Therefore when the zinc is added to the nitric acid, it alſo receives a part of the oxygen from the nitric acid. But then there is no combuſtion takes place, they produce the gaſeous oxyde of azote. But yet *this very ſelf-ſame oxygen*, which they ſay the marine acid takes from the nitric acid, produces (with the zinc) the moſt active combuſtion, but no gaſeous oxyde: ſee page 129. Here is a *huge chaſm* in your theory, Doctor: how is it to be ſwallowed? If the *ſervum pecus* gape ever ſo wide, their mouths will not be able to contain it. The idea of oxygen entering the lead, or mercury, without parting with its fixed fire, is not Dr. Thomſon's original thought; I publiſhed the ſame opinion in my Letter to Dr. Prieſtley, in the year 1788, but I relinquiſhed it in the year 1791, in my Treatiſe on Air.

But examine it another way. Conſider, reader, what wonderful *hocus-pocus* work they make of this poor hack of a conjurer, **Oxygen**: they make him teeming full of caloric, when in an aerial ſtate—but when condenſed in a ſolid ſtate, perfectly empty of caloric: this was Lavoiſier's theory. But our *little* followers of this wonderful ſyſtem-maker, have been forcing and twiſting poor Oxygen into a thouſand other ſituations. When the nitric acid is applied to metals, they ſteal its *condenſed* oxygen from it, and, nevertheleſs, forms it into nitrous air; and when another metal (iron, for inſtance) is applied to this nitrous air, it ſteals from it more oxygen; and, yet wonderful! the reſiduum (the gaſeous oxyde of azote) ſhall be now *cram-full* of oxygen, more than in any other body or ſtate; ſo as to admit candles, as Dr. Prieſtley obſerves, "to burn in it with double the flame;" therefore alſo *crammed* with double the quantity of caloric. Then here is a wonder of wonders! As the nitric acid loſes its oxygen and caloric, it ſhall in the ſame proportion become fuller of them! Juſt the identical ſame as *Fortunatus*'s purſe: the more was taken out of it, the fuller it was.—Still we have more wonders! As this condenſed oxygen is received from the nitric acid by the metal, the remaining acid is formed into an air. According to Lavoiſier, from whence came its caloric? Still, upon the ſame ſyſtem, the more money you take out of the purſe, the fuller it is! Juſt the ſame; the more brains you take out of our wiſe philoſophers' heads, the fuller they are! We have got ſo much out of many of them, I am afraid ſome of them have in conſequence *cracked*, from being too full.

But this Oxygen is one of the moſt extraordinary beings that ever philoſophy heard of. It is, according to Lavoiſier, *the principle of* ACIDITY, or *the* SOURING *conjurer;* as in proportion as the vitriolic, nitric, phoſphoric, carbonic acids, are united to him, they become acid. But as this whimſical Harlequin was not ſatisfied with this *ſouring* quality to two of the mineral acids, he takes it into his head to reverſe it in the third; and, inſtead of making bodies *ſour*, makes them *ſweet*. Thus nitrous air is *ſour*, but the gaſeous oxyde of azote is *ſweet*, which is an aerial body poſſeſſing the moſt oxygen of any one, if combuſtion is the teſt, making a candle burn in it with above twice the flame it does in pure oxygen gas. Alſo the marine acid, (the third mineral acid), is one of the ſtrongeſt acids in nature, which they *now* ſay does not poſſeſs an atom of the *conjurer*—not ſo much as the pareing of his nails, or a hair of his head. Yet by entering this marine acid, the acid loſes all its properties as an acid,—both in its attraction for alkalies and earths, nay, its action upon vegetables colours, (which were our *honeſt* forefathers' teſt of *acidity)*, not even turning them red.

But ſtill his *legerdemain vertigo* is more wonderful: in ſhort, you know nothing whether he is in a body or out of it. When you think you have eyes, noſe, mouth and ears to diſcover him, you are miſtaken: Harlequin's *feats* in jumping through windows, barrels and even key-holes, is nothing to him. As for inſtance, in changing vegetable colours; with our fathers he is ſaid to turn them all red, as that was *their* teſt of *acidity:* now he can vary his qualities as our modern gentlemen vary their faſhions; *(their* powers only extend to *the cut of their coat*, but our *conjurer* to his *qualities) :* he now turns all vegetable colours *white;* for when he enters this ſingular acid, (viz. the marine), he adopts the quality of changing bodies *white*, (I ſuppoſe to ſhew his *purity).* That is, when he has only immerſed himſelf up to the middle of the acid. But when he pops his head and ears into it, (what the *learned* call hyper-oxmuriatic acid), he then will not act at all upon vegetable colours. For he diſappears altogether, not turning them either red or white. The *Diable* an inch! not even a little finger of the *conjurer* can the vegetable colours attract; not even when the roſes, the jeſſamines, the carnations, &c. ſing him all their loveſongs which Dr. Darwen has taught them. Still he is inexorable. But to the animal kingdom he is far more cruel. As the roſes upon female Beauty's lovely cheeks are fed by him, by breathing his ſoft zephyrs into her inchanting breaſt. To receive him *in the ſtate of the oxmuriatic gas*, inſtead of fluſhing more heavenly her radiant cheeks, he turns them pale with the cold hand of Death. For he is ſuch a cruel raviſher, with ſuch rapidity does he leave the marine acid to ſeize her beauties, that in the ſhort conflict of four minutes he makes her a lifeleſs corpſe*! Reader, thou wilt enquire how this is done? Why, as our *aerial* philoſophers ſay, as they force their abſurd theories to account for every thing, the blood attracts its oxygen ſo rapidly as to induce ſo ſtrong an excitement as to take life. But it is a little wonderful, that, breathing the pure oxygen gas, no injury is done to the animal. How comes this, you *SOLOMONs?*——Your fooliſh hypotheſis will not ſerve you; viz. the oxygen gas poſſeſſing caloric, is not imbibed ſo readily. But does not the oxygen in the oxmarine acid, poſſeſs caloric, as it is a ſtronger ſupporter than any other body producing the moſt rapid combuſtion? But the hyperoxmuriatic gas does not deſtroy or burn the lungs, tho' the marine acid is double loaded with the oxygen in this gas, they ſay. How can this be explained? What difficulty! It is worſe than removing mountains. But the oxmuriatic acid gas will ſet fire to the animal oil: therefore we will ſuppoſe a fat man breathing this gas, inſtead of receiving oxygen, he would be only receiving an active and (indeed) *burning* acid. The proportion it contains, to the marine acid it is united to, is ſo very ſmall, that the common atmoſphere poſſeſſes a conſiderably larger portion, and which is eaſier attracted, for even diſtilled water can attract the oyygen from the azote of the atmoſphere. But it cannot do the ſame from the oxmuriatic acid. No; no bodies but highly combuſtible bodies. Of conſequence, the fat man, by breathing the oxmarine acid gas, would be all in a blaze. His *capers* would be in a higher ſtile than Dr. Boddoes's, or Mr. Davy's.

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* One would have ſuppoſed, according to what common ſenſe and our forefathers have taught us, that when a body is free as oxygen gas, it would act more powerfully than when it is chemically united to a ſtrong baſe, as azote in the nitric acid, or the marine acid in the oxmuriatic acid. Here this abſurd idea of its being owing to its being united to caloric, will not ſerve them; for they ſay, that both in the ſtate of nitric acid and the oxmuriatic gas, it is united to its caloric,—and in the latter, even in the ſtate of gas: therefore, in both theſe ſtates, it is not only united to caloric, but alſo to theſe *ſtrong baſes.* Then why does the oxmuriatic gas burn bodies *ſpontaneouſly*, and ſo violently, when oxygen gas will not? *Anſwer me this, ye* aerial *conjurers!* Nay, it will not act upon gold and platina, even in the ſtrongeſt heat, while the oxmuriatic acid acts in the loweſt. But, in reality, heat aids the action of bodies upon each other, even in its latent ſtate, as the ſteam of water, &c. upon iron; and in fifty other examples. (See my Letter to Mr. Cavendiſh.)——But, *aerial* philoſophers! condeſcend to come down from your *lunar* regions to our plain earth, and learn again the A, B, C. of common ſenſe. Nay, if you do not come voluntarily, I will drag you down—then, beware of the fall!—It will be ſuch ſcampering as "The Devil take the hindmoſt!"

But let us conſider it another way. Let us examine its action upon the moſt combuſtible body in nature, viz. phoſphorus. Phoſphorus, or ſulphur, when the nitric acid is applied to them, ſteals from them their phlogiſton or fixed fire forming nitrous air, but no combuſtion enſues. If nitrous air is applied to freſh phoſphorus, a combuſtion takes place. But if this nitrous air is formed into the gaſeous oxyde of azote, by having its acid more highly neutralized or ſaturated, becoming from *four* to *ſweet*, it will not act upon phoſphorus. Now let us examine the marine acid's action upon phoſphorus*.

But if the marine acid be phlogiſticated, ſo as to form the oxmuriatic acid, ſimilar to nitrous air; and phoſphorus applied to it, a combuſtion takes place. Again; if the oxmuriatic acid be more highly phlogiſticated, ſo as to get its acid perfectly neutralized, as in hyper-oxmuriatic acid, ſimilar to the gaſeous oxyde of azote. It will not act at all upon the phoſphorus.

Here then I think we may be allowed to ſay, that theſe acids act on the phoſphorus from their acidity; for when fully neutralized, no action takes place; and another more eſſential fact, it proves to us that bodies producing combuſtion upon combuſtible bodies, is owing to acid bodies having a certain ſaturation of phlogiston, or fixed fire: that as they attract the combuſtible bodies, fixed fire, their own fire is made free (or actual) during the efferveſcence, or induced chemical action; and by that means ſet the combuſtible bodies' fixed fire free as actual.

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* Dr. Prieſtley ſays, (vol. i. page 151.) "Charcoal abſorbed this air [marine acid] very faſt. About one-fourth of it was rendered immiſcible in water, and was but weakly inflammable.

"A ſmall bit of phoſphorus, perhaps about half a grain, ſmoked, and gave light in the acid air, juſt as it would have done in common air confined. It was not ſenſibly waſted after continuing about 12 hours in that ſtate, and the bulk of the air was very little diminiſhed. Water being admitted to it, abſorbed it as before, except about one fifth of the whole. It was but weakly inflammable."——How comes phoſphorus to burn in this acid, Mr. Cheveuix, as you ſay it poſſeſſes no oxygen?

"Putting ſeveral pieces of ſulphur to this air, it was abſorbed but ſlowly. In about 24 hours, about one-fifth of the quantity had diſappeared; and water being admitted to the remainder, very little more was abſorbed. The remainder was inflammable, and burned with a blue flame."——This air is the pure marine acid.

It is not confined to the acid, but other bodies, which have a ſtrong attraction for phlogiſton or fixed fire,—as the calces of gold, ſilver, &c. They alſo will attract ſo forcibly the fixed fire of ammonia, ſpirit of wine, &c. as to produce fulmination. All theſe bodies produce combuſtion far more conſiderably than atmpoſpherical air, which is formed of a mild acid and of a mild neutralization of fire, ſo that it will do for reſpiration, both to animals and vegetables,—by means of communicating to them its fixed fire, which is eaſily decompounded by their organs.—*(See my former works.)*——But none of theſe airs will do for animal and vegetable life, but atmoſpherical air: even the oxygen gas of the elaboratory will not anſwer by any means ſo well, even if mixed with the ſame proportion of azote,—as I have indiſputably proved. The atmoſphere's fire is ſo looſely attracted, that even the electric ſpark can ſet it looſe as free fire, forming the atmoſphere partly into fixed air;—which was performed both by Bergman and Prieſtley.——The atmoſpherical air produces combuſtion from its fixed fire being ſet looſe, and not from its acid having a ſtrong attraction for the fixed fire of the combuſtible body.

Now I think my reader *muſt* ſee how beautifully MY ſyſtem accounts for all the phenomena; and he certainly will not deſire me to ſhew how groſs THEIR explanations of the ſame phenomena are:—No: I am certain he can ſee that himſelf.

I am, however, anxious to make my doctrine as plain as poſſible, that even the moſt ignorant will be able to explain the moſt minute chemical action in theſe proceſſes. Thus: Why the oxmuriatic acid is ſo powerful a calciner or burner of bodies, is owing to its poſſeſſing fire in a ſtate eaſily made free or actual; thus, when the calx of lead and manganeſe form it, their fire is in a looſe ſtate. Alſo, when the marine acid receives the nitrous vapour, (ſee page 119), its fixed fire is in a more fixed ſtate. But then it is united to another moſt active acid for combuſtion, viz. the nitrous; and when there are two acids acting in any chemical proceſs, they greatly promote combuſtion; as, in the oxmuriatic of potaſh, by adding the muriat to vitriolic acid, an active combuſtion is produced *. Now how is this produced, accord-

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* Mr. Fourcroy found that alkalies, when united to the dephlogiſticated marine acid, are capable of being ignited, the ſame as any combuſtible body.—" When M. Fourcroy poured the concentrated ſulphuric acid on the oxygenated muriat of potaſh, he obſerved a violent efferveſcence, and the production of a white vapour, which, though it reſembled in ſmell the oxygenated muriatic acid, had a character peculiar to itſelf: the ſalt and the acid both became of an orange colour. He then varied the experiment, by caſting ſome of the muriat into the acid: the efferveſcence thus occaſioned, was ſcarcely perceptible: but when the mixture was ſtirred with a glaſs tube, a violent exploſion took place, accompanied with tranſient flaſhes of red light; after this commotion had ſubſided, a ſecond agitation produced another exploſion not leſs violent, and accompanied with more ſplendid corruſcations than the former. On bringing the flame of a taper near ſome of the mixture, which had already made one exploſion, white vapour aroſe from it in great abundance, and a detonation took place, which broke the veſſel that contained it, and was ſucceeded by a number of partial exploſions of the parts of the mixture which were diſperſed to a conſiderable diſtance. A piece of phoſphorus, immerſed in this vapour, took fire, and occaſioned a detonation ſtill more violent. A mixture of this muriat with the concentrated nitric acid, produced ſimilar phenomena in a yet greater degree.

" Theſe phenomena M. Fourcroy recommends to the attention of philoſophical chemiſts; he aſcribes them to the ſudden and ſimultaneous ſeparation of light, condenſed vital air, and oxygenated muriatic acid, from the muriat."—See the Appendix to the Monthly Review for Auguſt 1792, page 514.

ing to *their* theory? Here is all oxygen, (or *ſupporter)* and no combuſtible; yet how *highly* combuſtible is the mixture? If the cauſtic potaſh contains free fire, which it received by the calcination, yet when it united to the moſt active acid for combuſtion, viz. the oxmuriatic acid, it muſt have parted with its fire. Then, from whence proceeds thoſe violent exploſions and ſplendid corruſcations of red light, (what I call FIRE LIGHT), by merely ſtirring the mixture? Mr. Fourcroy's explanation is truly laughable, Dr. Thomſon. They teach us that oxygen, in being condenſed, parts with its caloric: but here they gravely tell us, it is from the condenſed oxygen being ſet free, or made aerial again. I ſhould have ſuppoſed, Dr. Thomſon, (in reference to your *tinkering* this old French *kettle)*, that as you would have the Oxygen to take its *dear* Caloric, when condenſed, along with it in ſome few caſes, your *tinkering* did not extend to when it was ſet free again, that active light and heat would come from it. That is juſt as much as to ſay, that this *hocus pocus* conjurer's *kettle*, when it was placed upon the fire the water within it would *freeze*, but when taken off the fire it would *boil!* This is truly the caſe, neither more nor leſs, and they that diſpute it poſſeſs not an atom of common ſenſe. But I muſt charge *myſelf*, with demeaning my pen in expoſing ſuch abſurdities. If they want common ſenſe, why ſhould I have the labour to hammer it into them?

But let me explain it upon juſt chemical principles. The vitriolic acid attacks the alkali, as having a greater attraction for it than the oxmarine acid. In the chemical action induced, the fixed fire of the oxmarine acid is ſet looſe, they both then attack the alkali and ſets looſe a part of the fixed fire it is formed of. Need we be ſurpriſed at this, when we know the action of gunpowder. The combuſtion of the charcoal and ſulphur gives ſuch activity to the nitrous acid, as to ſet a great part of the alkalie's fire free. When the oxygenated muriat of potaſh is uſed inſtead of nitre, it is more active, and the alkali is more decompounded.

Therefore this informs us, that when the oxmarine acid is formed by the nitrous vapour, that the nitrous acid aids the combuſtion or calcination of metals, from its aiding the marine acid in ſetting the phlogiſton looſe as free fire, and ſo getting activity to act upon the metal's phlogiſton. That fire aids the calcination of metals we have a ſtriking inſtance of, in the ſulphuric acid:—it will not calcine metals, without water to aſſist it; but aid the acid by heat, and it will calcine the metals forming the vitriolic acid air. Now this is the reaſoning of common ſenſe, you *aerial SOLOMONs!* which you are not acquainted with.

I have ſhewn that the white or grey calx of lead become red by long calcination, it requiring, according to Dr. Gren, 48 hours. I ſuppoſe that in this proceſs the lead is receiving fire; they ſuppoſe oxygen—but let us examine it. Dr. Priestley ſays in the Summary View of his Principal Facts,—" All metallic calces have a strong affinity with the nitrous acid, and when united with it becomes white," (vol. v. page 236.) He alſo ſays, that " nitrous vapour is imbibed by red lead, which then becomes white," (vol. iii. page 230.) And he ſays, that when the red lead imbibes this vapour, it " changes it " into a perfectly *white and brittle ſubſtance*, at the ſame " time heating it exceedingly."—Now let us examine theſe important facts by Mr. Lavoiſier's hypotheſis and mine. The nitrous vapour, as containing ſome fixed fire, and in conſequence, according to my theory, better adapted for combuſtion, ſeizes rapidly upon the fixed fire that the minium had imbibed by its calcination, and ſets it looſe as free fire, "heating it exceedingly." But then it may be ſaid, that this is done by this vapour attracting the lead's oxygen. But Dr. Prieſtley expelled this vapour from the lead by heat, and it was the ſame red nitrous vapour, not at all altered: (ſee vol. v. page 241.) But when the nitric acid is applied to the metals, viz. "lead, zinc, copper and tin," they immediately became white, or the nitrated calces. The acetic acid does the ſame with lead, forming a white calx. Here, in this caſe, they have received their full ſhare of oxygen from the acids, according to *their* hypotheſis. But if my reader is ſtill a ſceptic, let him take the white lead,—or the calx formed by the acetic acid,—or the grey calx of lead,—or maſſicot, the yellow calx of lead,—and calcine them in cloſe veſſels, and he will equally make minium*.

Our forefathers have been juſtly ridiculed in their mercenary and abſurd attempts in making gold; but our modern *aerialiſts*, (from their *deep*, *profound*, and *pene-*

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* Bodies become white from being bereft of fire. Thus, to blanch vegetables exclude them from the ſun. Dr. Prieſtley ſays in his 'Facts,' "Heat deepens the colour of this [the nitrous] acid," (vol. iii. p. 249.)—And again, "Its colour is univerſally owing either to phlogiſton or heat," (vol. iv. p. 2.)—Mr. Smithſon makes the ſame obſervation on the calces of metals, (ſee p. 121.) And "it cannot be from receiving oxygen," as Doctor Prieſtley ſays:—the metals, lead, copper, zinc and tin, all become white by calcination in the nitric acid,—which THEIR theory ſays, is *by receiving oxygen;* but MY theory ſays, *by being dephlogiſticated.* And, reader, what does THY ſagacity ſay?

trating ſagacity) have carried their projects conſiderably higher—to the making of diamonds! which is, according to their theory, the moſt *ſimple proceſs*—Charcoal is diamond! but only having got a little ſaturation of their conjurer, *Oxygen;* and it is only coaxing the rogue to leave his neſt, and behold diamonds appear! ═══ Now let us examine their table of attractions.—Phoſphorus attracts oxygen in any temperature from the air, and diamonds require the higheſt temperature;—then doſe the dog well with phoſphorus, and he muſt leave his den; or with pyrophorus. In ſhort, chemiſtry is full of bodies capable of doing it: nay, Mr. Muſhet ſays that carbon, in ſome of his proceſſes, entered iron in the ſtate of diamond. O "*cunning little ISAAC!*" I expect to hear that he will turn his iron foundry into a diamond one. —*Then at it, my boys!* and we may pave our ſtreets with diamonds. It will ſave the poor wretches, whom the avarice of man hath forced into the mines to ſeek this precious mineral. Pugh! the old alchemiſts were a *bagatelle* to our *SOLOMONs*.

Dr. Thomſon, I obſerve, will not allow that gold, ſilver, and mercury are combuſtible bodies. What ſtrange aſſertions this theory of his forces him upon! It is ſingular that theſe metallic bodies ſhould differ ſo wonderfully from the reſt. But let us put them to the teſt. . . . The eſſential oils, or any other phlogiſtic body, will precipitate gold from its ſolution in *aqua regia*, in its metallic ſtate. Dr. Prieſtley and the American philoſophers, have clearly ſhewn that you cannot reduce the vitriolated mercurial calx, without hydrogen gas. But why do I waſte my time with anſwering ſuch abſurdities? If iron or zinc are diſſolved in a nitrous ſolution, nitrous air, or the gaſeous oxyde, is evolved. In a nitrous ſolution of ſilver or mercury, nitrous air is evolved, and no ammonia appears. The only appearance of ammonia is, when nitrous air is made into the gaſeous oxyde of azote, by attracting the phlogiſton of ſome freſh iron, and this gaſeous oxyde, in time, loſes its phlogiſton which it had received by the two calcinations, being left in the ſtate of azote gas, and the phlogiſton becoming ammonia. But if zinc is diſſolved in a nitrous ſolution, as it is ſo highly a combuſtible body, (ſee note, page 117) as Dr. Gren obſerves, it will form in one calcination this gaſeous oxyde of azote. But even in this very combuſtible body, ſo full of fixed fire, if the nitrous acid is much diluted, the zinc only then forms the nitrous vapour, as Doctor Prieſtley found: (ſee note, page 115.) Ammonia is eaſily diſcovered by its ſmell, therefore Dr. Thomſon might eaſily diſcover it by his noſe, if it was to be found in the nitrous metallic ſolutions, where nitrous air is formed.— But let us more minutely examine the acid ſolutions of zinc. The vitriolic acid and water, produces (with it) hydrogen gas; but the nitrous ſolutions, either the gaſeous oxyde of azote, nitrous air, or nitrous vapour, juſt as you dilute the acid with water, or apply heat or cold to the ſolution. But the reſiduum is the calx of zinc, and the nitrous acid — juſt the *identical ſame* reſiduum, when the vitriolic acid and water are uſed; only in one caſe, the vitriolic acid, and in the other the nitrous; but *no* ammonia in either. Then let us examine theſe airs. The oxmuriatic gas, with theſe airs, may be ſet on fire. Their phlogiſton being turned into free fire, how comes this—the one, (viz. the nitrous airs) ſupporters; and the other, (the hydrogen airs) combuſtibles. Dr. Thomſon and Mr. Lavoiſier ſay, that the one is hydrogen and the other oxygen. But here the oxmuriatic gas makes all theſe airs combuſtibles,—and becomes after the proceſs, marine acid. Heavens! what a Lob's-pound theſe facts lead our *acrialiſts* into! and how the *Diable* can they extricate themſelves, thinkeſt thou, reader? For, Doctor Thomſon, the nitrous airs, your ſupporters, ſhould all have been formed of caloric, and the hydrogen of light; and this all from the zinc ſolutions. But then, examining the ſolution after theſe airs are evolved, they are identically the ſame. This is truly *legerdemain* work!—The combuſtion of all the nitrous airs with the oxmuriatic gas, ſhould only have produced caloric: but both fire and light are produced with the gaſeous oxyde of azote.

Mr. Kirwan, in his Eſſay on Phlogiſton, page 31, ſays, "If ſulphur be digeſted in oil of turpentine, and then ſlowly diſtilled for ten or twelve days, it will be converted into vitriolic acid, according to *Homberg. Mem. Par.* 1703. It muſt be remembered, that if this experiment be not cautiouſly conducted, it is very dangerous."

Now how is this fact to be explained by *their* hypotheſis? Here are high phlogiſtic bodies burned without oxygen, and an acid formed. It is truly laughable.——The hyper-oxmarine acid is formed from the fixed alkalies, could they give it oxygen, or even caloric, according to their hypotheſis. But according to mine, fixed fire—alkalies being formed of fixed fire of a leſs concentration than phlogiſton. On the other hand, when the nitric acid turns ſulphur into the ſulphuric acid, and nitrous air is formed, then what comes of the light? (for nitrous air is a ſupporter, the Doctor ſays, and alſo forms nitrous air into the gaſeous oxyde of azote.) Here, like the conjurers, the light muſt have eſcaped through the key-hole. Alſo, when hydrogen gas is formed from the metals and the vitriolic acid and water, what forms the gas? Mr. Lavoiſier ſays, caloric forms the gas: but, hear a new doctrine;—*LIGHT forms it.* But if nitrous gas be formed from the ſulphur, then *light* muſt form it. But then it is a *ſupporter*. . . . . In ſuch a way one might ring the changes without end!——But I hope I have expoſed its abſurdities ſo clearly, that even the *ſervum pecus* cannot miſtake them.—Oh, what a noble taſk has the philoſophical world forced me to! It is like cleaning the Argean ſtable.

But, Mr. Chevenix, let us examine the action of the marine acid upon phlogiſtic bodies, according to Doctor Prieſtley's experiments, quoted in the note, page 152. This is the marine acid exhibited in the form of air; but water condenſes it into the pure marine acid. Charcoal produced one-fourth of it into a weak inflammable air, ſomething ſimilar to Mr. Cruikſhanks' gaſeous oxyde of carbon: the one poſſeſſing the aerial acid or fixed air, and phlogiſton or carbon; and the other, the marine acid, and phlogiſton or carbon: for if they are fired with oxygen gas, the one will give a reſiduum of the marine acid and fixed air, and the other double the quantity of fixed air, as Mr. Cruikſhanks found. Phoſphorus burned in this marine acid air, and therefore leſs of it was formed into this inflammable air,—only one-fifth of the whole, the remainder of the acid entering the phoſphorus. But with ſulphur, as it is a leſs combuſtible body, therefore the phlogiſton the marine acid air received from it, was not ſet free as free fire, but four-fifths of it formed an inflammable marine acid air, more highly inflammable than the two others; that is, having a greater proportion of phlogiſton; and, as the Dr. ſays, "burned with a blue flame."

Now, how is it poſſible to *reconcile theſe facts with their theory?* From whence comes the weak inflammable air, formed by the marine acid and the charcoal? and alſo the inflammable air from the phoſphorus and the ſulphur? —I think the TYRO will ſcarce require me to ſhew how perfectly inadequate THEIR theories are to account for theſe phenomena, and how beautifully and rationally MINE account for them: for he will be able to do that *himſelf*, with the aid of COMMON SENSE; and unleſs he takes this ſolid old-faſhioned gentleman along with him in his philoſophical diſquiſitions, he will be bewildered with *aerial nonſenſe.*

Whatever way we examine this theory, abſurdity upon abſurdity ſtare us in the face.—The calces of metals receive *by far the greateſt weight*, by being calcined with the ſteam of water;—as iron, for inſtance. Then certainly it ſhould form the oxmarine acid with this great overplus of oxygen—*but it will not;* particularly as this oxygen is ſaid to poſſeſs no caloric, it being received from water, and therefore ſhould, according to them, be more eaſily attracted by the marine acid. But take the ſcales or calx of iron, formed by a ſmith's forge, and calcine them ſtrongly with a ſtrong heat; and though this calx does not poſſeſs one-fifth of the quantity of oxygen, (according to their weight), it will give the marine acid the ſmell of the oxmarine acid; and that this calx of iron got by the calcination *fire*, is clear; for, as Mr. Keir obſerves, if the ruſt of iron is diſtilled, it will form ammonia, it concentrating and fixing ſo much fire in the proceſs. That FIRE, *that* Great Agent, the Soul and

Life of the Univerſe, ſhould be conſolidated and unite itſelf with various bodies, is a rational theory, and is forced upon us by all the phenomena in nature; and I am ſurpriſed it is not univerſally received: But it was driven from our philoſophy by Black's doctrine of latent heat.

Now I think I cannot impreſs my reader too ſenſibly with the action of the acids upon combuſtible bodies,—phoſphorus, for inſtance. The nitric acid attacks it, and takes all its fixed fire or phlogiſton from it; which is, according to the doctrine of our honeſt forefathers, the nitric acid being placed at the head of the column of phlogiſton. Therefore I have taken for my example the moſt combuſtible body, and the acid of the moſt attraction for the combuſtible body. The nitric acid ſeizes upon all its phlogiſton, and forming with it nitrous air. But this nitrous air, by being united to a half ſaturation of phlogiſton, when applied to freſh phoſphorus attacks it. But in this chemical action combuſtion is produced, and their phlogiſton is ſet free, giving both light and heat. This chemical action of combuſtion is clearly produced by the phlogiſton of the nitrous air being ſet free by the fermentation or induced chemical action, and by that means its free fire gives that activity to the induced chemical action, ſo as to ſet free the phoſphorus's phlogiſton, as free fire. It is this ſame chemical action which is produced by the fermentation of vegetable bodies. Vegetable bodies, when immerſed in water, as the water enters them it diſſolves or ſeparates their texture, and, in conſequence, they attract the oxygen of the atmoſphere,—which attraction, or chemical action, ſets partially and gently looſe the fixed fire of the oxygen, as free fire, which aids and gives life to the fermentation, ſo, as in the acetous fermentation, not only the oxygen's fixed fire is ſet free, but alſo the vegetable's; and the oxygen's acid and the vegetable's acid are left in the ſtate of a concentrated acid, as vinegar. But in the vinous fermentation, or chemical efferveſcence, both the oxygen's fixed fire and alſo the vegetable's fixed fire, which were in the ſtate of ſugar, (or the ſaccharine quality), is more concentrated in the ſtate of *alcohol*, but a very ſmall part of their fire being ſet free—only enough to induce the chemical action, or efferveſcence: for if you by art, during their efferveſcence, lower their temperature too much, you ſtop the proceſs; or if you raiſe the temperature too much, you equally injure the working of the vat, hindering that gentle accumulation of, or centring and fixing their phlogiſton. And during this fermentation, a great part of the acid, (as the ſaccharine for inſtance) is ſet looſe as an aerial acid, as fixed air.—To promote this fermentation, or induced chemical action, yeaſt is employed—which is a body formed of the eſſence of this fermentative matter; ſo that it begins its action immediately, and haſtens the proceſs. It acts juſt the ſame as when one employs free fire, to ſet looſe the fixed fire of the atmoſphere, and a combuſtible body's fire. In theſe two fermentations the action is a proceſs of concentrating the *acid* principle, and the other (the vinous) is a proceſs for concentrating the phlogiſtic or *fire* principle. But in both caſes there is as much fire or phlogiſton reſerved as to form the vinegar, and the other (the vinous) as much of the acid principle as to form the *alcohol* to fix the phlogiſton. For, in all caſes where phlogiſton is ſtrongly concentrated, it is by the acid principle. As in ſulphur, formed of the vitriolic acid and phlogiſton (or fixed fire), the ſame in phoſphorus, the phoſphoric acid and fixed fire; and in ſugar, the ſaccharine acid and fixed fire; and in *alcohol,* the vegetable acid and fixed fire: and ſo throughout nature.

Therefore we ſhall lay down this general rule, that in order to produce a ſtrong chemical action, either as combuſtion, fermentation or putrefaction, a body is required formed of fixed fire and an acid: *(yeaſt I ſuppoſe is formed of theſe two bodies).* Therefore oxygen is formed of theſe two bodies; ſo is nitrous air, and the gaſeous oxyde of azote, and ſo is the oxmuriatic gas. Therefore they bring on ſuch ſtrong chemical actions, that in the proceſs their own fixed fire is ſet looſe; and fire is the great promoter of all great chemical actions: but, as I obſerved before, the nitric acid only produced a decompoſition of the phoſphorus, attracting its phlogiſton, and communicating to it a part of its own acid. But nitrous air induces this chemical action of combuſtion; and that an acid is required in this chemical action is clear; for if this nitrous air has its acid fully neutralized, ſo as to turn from ſour to ſweet, as in the gaſeous oxyde of azote, it will not act upon phoſphorus, either in taking from it its phlogiſton, or in ſetting fire to it. But in ſulphur, which is a far leſs combuſtible body, the nitrous air does not ſet it on fire, but only receives as much phlogiſton from it as to neutralize its acid to form the gaſeous oxyde of azote.—But even the gaſeous oxyde of azote will promote combuſtion, and that in the moſt violent manner, as it poſſeſſes ſo ſtrong an acid, and more particularly ſo high a ſaturation of phlogiſton, or fixed fire. But then, as it has no unſaturated acid to begin the chemical action, it muſt be done by actual fire, as the placing a burning candle in it.

But reſpiration and putrefaction is a very different proceſs from all theſe, and none of theſe airs will anſwer for them, but the atmoſpherical air—not even the oxygen gas of the elaboratory very well. The animal blood will not be able to decompound theſe airs, take from them their fixed fire; and their fixed fire would not anſwer; the animal organs would not be able to ſet it looſe as free fire, it being too much fixed or concentrated.

That this theory is true, accounting ſo beautifully and rationally for all the phenomena, (not one phenomenon excepted); ſo very different from the French theory, it not accounting *rationally*, according to the principles of *common ſenſe*, for one of the phenomenon. As ſhewing this in all theſe phenomena, would be much too tedious, I will only juſt examine them by one; and as I have ſeldom quoted my own experiments, (for that was one of the ſhafts by which they attacked my doctrines, aſſerting that ſome of my experiments were wrong); therefore in this work I have brought their own, to prove *my great, extenſive, and important ſyſtem.*

(☞ Dr. Prieſtley's experiments quoted in page 119.)

Here then (he ſays) " the marine acid, by imbibing a little of the nitrous vapour, becomes poſſeſſed of all the properties of that *aqua regia*, which conſiſts chiefly of ſpirit of nitre, and in much greater perfection. For while the common *aqua regia* will hardly diſſolve gold without the help of heat, this diſſolves it with as much rapidity as I have almoſt ever ſeen in any chemical ſolution whatever, when it is perfectly cold. The quantity of gold that this *aqua regia* is capable of diſſolving, in proportion to its bulk, I have never aſcertained; but it ſeems to exceed the common *aqua regia* very much in this reſpect." — Now, for the ſake of Reaſon, Common Senſe, Truth, Honour and Juſtice, let us introduce ſome kind of argument into theſe diſquiſitions.—The French theory ſays, this oxmuriatic acid is formed by the marine acid receiving oxygen from the nitric acid. But then it clearly appears that a *very ſmall* proportion of nitrous vapour produces a far ſtronger *oxygenation*, as they term it. Let us for a moment reflect upon what this nitrous vapour is formed by, viz. the nitrous acid and biſmuth, which calcines the metal; that is, according as it loſes its oxygen, this nitrous vapour is produced; and this deoxygenated vapour having ſuch a ſtrong effect upon the marine acid, ſo as to produce ſo ſtrong an oxmuriatic acid, and the ſmall quantity of nitrous vapour required: ſo, as Dr. Prieſtley ſays, "I did not perceive that the bulk of the marine "acid was ſenſibly increaſed by the proceſs[*]." — Now,

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[*] In theſe diſquiſitions our French theoriſts do not mind exact calculations; a difference, for inſtance, of 20 to one, they would ſuppoſe was lowing to ſome error in the conducting of the proceſs, though not in their "heaven-born" theory. But I think that the difference that the oxygen which the ſmall quantity of the nitrous vapour could contain, would not be the millionth part which the nitric acid contains of oxygen, *in the common* aqua regia. But here the millionth part gives more to the marine acid, than the million for it makes a ſtronger oxmuriatic acid. But ſtill, allowing this conceſſion to the wonderful, WONDERFUL, WONDERFUL Theory! yet we are ſtill aground. For the marine acid is not capable of taking the ſame quantity of oxygen from the nitric acid as metals do, as they would then form it all either into nitrous vapour, nitrous air, or the gaſeous oxyde. How the *Diable* can you get your friends out of this difficulty, Mr. Reviewers? Oh, by a ſingle daſh of your pen, and the *ſervum pecus* gaping wide, it is ſwallowed before you can ſay *Jack Robinſon* or *Tom Fool.* They are ſo well trained, they can ſcarcely reliſh Common

Meſſieurs AERIAL PHILOSOPHERS! you may endeavour to make us believe that the MOON is a large CHESHIRE CHEESE, and I really believe you will get the Reviewers to countenance it, and the *ſervum pecus* to *ſwallow* it! —But Common Senſe muſt return to our philoſophical diſquiſitions, againſt all the mean arts to hinder it.

Reader! will it be neceſſary to inform thee, that the nitrous acid received the phlogiſton from the biſmuth, and, in conſequence, phlogiſticated the marine acid; and that when a freſh metal,—the zinc, for inſtance,—is applied to this phlogiſticated marine acid, the acid attacking the zinc, the phlogiſticated nitrous vapour's fire is ſet looſe during the calcination of the zinc, and which gives ſuch an active operation to the calcination, ſo as to ſet its phlogiſton alſo looſe as free fire; ſo that there is no phlogiſticated air (that is, what they call the gaſeous oxyde of azote) produced; but only the mixture greatly heated, and the acid partly phlogiſticated. Nay, in the oxmuriatic acid, made by the diſtillation of lead or manganeſe, the acid receives ſo much fixed fire from them, and by diſtillation, and in that looſe ſtate ſo as to ſet the zinc actually on fire, ſpontaneouſly. That, adding phlogiſton to the marine acid, will not form this oxmuriatic acid; for the nitrous acid which is united to the phlogiſton, ſeems to be neceſſary. That as the chemical action of the marine acid acting upon the metal, the gold, platina, &c. the nitrous acid attacks its phlogiſton, ſet-

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Senſe; it is quite inſipid. Like the children of the Greenlander, from being fed with *ſtinking train oil,* when they are offered a piece of Engliſh beef, they throw it away, as being quite inſipid. I think you ought to be put upon this train oil regimen, till you are open to the voice of Truth, Juſtice, Honour and Common Senſe.

ting it looſe as free fire. There is juſt the ſame action induced upon the oxmuriat of pot-aſh, as I have ſhewn, (ſee page 154.) That as the vitriolic or nitric acids attack the alkali, they give that chemical action to the oxmuriatic acid to attack the alkali ſo as to ſet its fire looſe. Percuſſion does the ſame — it induces the ſame action upon the ſalt: it being well known that it detonates by a violent blow of a hammer. Now, reader, I flatter myſelf that thou muſt be ſtruck with this beautiful and clear reaſoning, which does no violation either to Common Senſe or Truth, but accords with both, and alſo makes no ſacrilege upon the *ſolid* opinions of our *honeſt forefathers*, who were no *aerialiſts*[*]. That the nitrous vapour in the marine acid ſeems to act as the atmoſphere does in combuſtion or calcination, or as nitrous air does upon phoſphorus, or the gaſeous oxyde does in common combuſtion, — or as the oxmuriatic acid does, (formed by the acid being united to the fixed fire and earth of the lead and mangane ſe); in all theſe caſes, their action being the great agent to the combuſtion or calcination, in producing fire to give activity to the chemical actions. But not one of theſe, but atmoſpherical air, will do for animal or vegetable life. But if the French theory was true, they would all do for their life, as well as for combuſtion and calcination, by giving their oxygen to animals and vegetables. But it is truly ridiculous.— *O tempora! O philoſophia!*

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[*] Now all the ſenſe that I require of the *ſervum pecus*, to detect the groſs abſurdity of the French theory, is to be able to count their fingers; for if they are able to do that, they muſt be ſenſible how truly ridiculous it is in accounting rationally for the phenomena. Mr. Briſted ſays, in his Tour, that Sir Joſeph Banks has obtained fame by counting his fingers: But I wiſh many who have obtained it, had obtained it ſo innocently. I ſhould ſuppoſe that when *aerial* philoſophy has come to her *ſenſes*, (for at preſent ſhe is under a French *mania*), there will be a black reckoning with many; inſomuch that they may hereafter think, they had better been innocently employed as counting their fingers!

We have entirely loſt ſight of all the ſolid rudiments of our forefathers, for the *aerial* French flights of modern chemiſtry. I am certain Engliſhmen could not have invented ſo *aerial* and *wild* a theory:—it required the warm imagination of French men.—If vitriol of iron is calcined ſo as to have all its water of cryſtalization evaporated, and then diſtilled "with a reverberatory fire for three days, (without intermiſſion)," the vitriolic acid is forced from the calx, and it remains in the veſſel in the ſtate of colcothar, or red calx of iron; but it requires an immenſe ſtrong fire. If alum is treated in the ſame manner, its vitriolic acid is alſo drove off, and its earth is left in a very acrid and cauſtic ſtate. Alſo, if limeſtone is treated in the ſame manner, its acid fixed air is expelled, and the earth alſo left in a cauſtic ſtate. What this cauſtic ſtate is:—Apply water to the cauſtic lime, and fire is expelled from it: juſt the ſame with the other calcinations, which I might prove by a thouſand proofs. Now this colcothar, or red calx of iron, as Mr. Stott obſerves—"All my experiments, however, concur to ſhew that muriatic acid, whether concentrated or diluted, acts but weakly upon metallic bodies in general, ſo long as it remains common muriatic acid, and produces no ſenſible effect whatever, either upon antimony or quickſilver; but no ſooner does it meet with manganeſe, or red oxyde of iron *(vulgo colcothar)*, then it becomes oxygenated, and preſently oxydes---metals on which it exerted no powers before."——Now, reader, reflect how this colcothar is made in cloſe veſſels, it being the method of making the acid of vitriol, where this conjurer *Oxygen* is excluded. But this calx of red iron is by no means ſo heavy as that which is made by ſteam, therefore cannot, according to the French principles, poſſeſs by any means the quantity of oxygen which the other contains; yet the one oxygenates the marine acid, but the calx of ſteam does not. Need I ſay any more?

We muſt be all ſenſible, that in thoſe nice chemical proceſſes of airs, how very uncertain and deceitful they are; and more particularly, how the *great theoriſts* are anxious to bring their experiments to tally with their theory. For inſtance, in Mr. Lavoiſier's experiments of reducing the calces of metals with charcoal, he ſays "only fixed air was produced:" now it is '*found*' that 'four-fifths of this is inflammable air.' Juſt the ſame in their pompous experiments of producing water in the firing of oxygen and hydrogen gaſſes. The difficulty of aſcertaining the exact quantities and weights of theſe airs conſumed,--- the vanity of eſtabliſhing their wonderful theory,--- the far too great a ſcale it was upon,--- their anxiety for the revolution it would produce;--- on all theſe accounts I diſpute their experiments *in toto*. Dr. Prieſtley, who, I affirm, was the firſt experimenter, found ſuch different reſults when he accurately fired a ſmall quantity of theſe airs, and which he accurately weighed, and the quantity of acid he found in the proceſs:—ſo that he ſays, in his '*Doctrine of Phlogiſton eſtabliſhed*,' (page 48.) "With the greateſt confidence Dr. Maclean ſays, 'The denſe acid 'vapour that I produced by the exploſion of the two 'kinds of air, was occaſioned by the azote contained in 'the oxygenous gas that I employed.' He might as well have ſaid, (continues Dr. Prieſtley) it was occaſioned by that I did *not* employ. If ten times the quantity of azote in the air I uſed had been wholly decompoſed, it would not have amounted to the hundredth part of the weight of the acid that I procured."

That the loſſes which were found in the different proceſſes clearly and fully prove, that in expoſing bodies to great heats, their ſubſtance was in part diſſipated, as in the experiment of firing hydrogen and oxygen gaſſes, and in Mr. Muſhet's late experiments in reducing metals*; but more particularly in the Galvanic experiments, where the neutral ſalts in the diſks really loſt their acids. And as we know that heat will penetrate all bodies, we have a right to ſuppoſe that *that* heat, when united to other bodies, will carry along with it thoſe bodies; that in the electric fluid we know that it poſſeſſes ſomething more than mere FIRE, or *heat* and *light*, from its powerful force, and from its phoſphorus, and ſulphureous ſmell. This ſenſe of ſmell, our chemiſts admit its teſt in chemical proceſſes, as the ſmell of oxmuriatic acid and of the nitrous: when theſe appear, they allow the proof of thoſe bodies exiſting in the proceſs; then why not the acids, from their ſmell in the electric fluid?

That the acids may be tranſmuted, appears from many proceſſes†.

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* Where charcoal diſappeared in cloſe crucibles, if the crucibles poſſeſſed any moiſture. Alſo the French experiments on diamonds, where they diſappeared in cloſe crucibles.—See Mr. Muſhet's new Experiments in the Philoſophical Magazine.

† Dr. Prieſtley ſays, (vol. ii. page 161.) "The celebrated Mr. Stahl obſerved, that by diſtilling iron in the marine acid, ſome nitrous acid was produced. Mr. Woulfe informs me, that he has done the ſame by a proceſs different from that of Stahl; and alſo that he has converted

That in firing oxygen gas and hydrogen gas, Dr. Van Marum found that they required eight hours conſtant combuſtion; for, if done in leſs time, there was always an acid in the reſiduum. Now, as I have incontrovertibly ſhewn, that the acid is attracted by the fire, and eſcapes with it, as ſeen clearly in the neutral ſalts, expoſed to the diſks of Sigr. Volta's pile, where the acid all eſcaped with the electric ſpark, and even it took with it the groſſeſt bodies, as the calx of the metals, and any other expoſed to its influence. And in the firing of theſe airs, all the oxygen was of that great purity ſo as contain no azote. Dr. Prieſtley found nevertheleſs that the water was acidulated. And when he added azote to theſe gaſſes, inſtead of more acid there was leſs;—— clearly upon this principle; it retarded the active combuſtion which is neceſſary to forming an acid, otherwiſe they form fixed air and azote. Now as water, free from acidity, can only be formed by this very long continued firing, *(as is allowed even by them)*,—does it not clearly appear it muſt be from the fire attaching itſelf to the acid and eſcaping with it?

The French philoſophers ſay, that the reſiduum of water is equal to the weight of both airs: I ſay, that all the experiments of other philoſophers demonſtrate the contrary. That theſe airs loſing their fire, would make their conſtituent bodies become heavier,---as fire, from the principle of repulſion, makes bodies become lighter;

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the acid of nitre into the marine acid, which I believe was never done before.

"This I conſider as a capital diſcovery; and whenever this excellent chemiſt ſhall think proper to publiſh his proceſs, it will, I doubt not, be a great means of advancing the bounds of natural knowledge."

I have fully ſhewn. But upon cloſe examination, moſt, (if not all) of the French experiments have been found erroneous; and as to theſe upon the producing water, their apparatus were ſo complicated as not to admit of an accurate examination of the reſiduum. Beſides, I declare, that, in ſupporting their theory, their conduct has been ſo warm and ſtrenuous, that their zeal greatly outſtripped their accuracy. This has been fully ſhewn in the repetition of moſt of their experiments; and every man who has accurately attended to their experiments, muſt have remarked that *moſt of them have been refuted.* Indeed, depending upon *data* in reference to theſe *aerial* experiments, have been ſo very precarious, that no certainty can be expected from them. Even in the burning of charcoal, a ſolid body, Mr. Lavoiſier has been erroneous; as Mr. Cruikſhanks aſſerts, that the weight of the fixed air is not equal to the weight of the charcoal and oxygen gas, before the combuſtion. But men, anxious to eſtabliſh a theory, do not reſt upon trifles, but make all ſmooth, being under a ſtrong bias: therefore *their own judgment is miſled.* And even in producing water, most of their experiments make the reſiduum ſomething leſs than the airs burned.

But every one must acknowledge, that in theſe very complicated and obſcure experiments there must be great doubt. But the ſuppoſed compoſition of water ſhould be accurately examined in other experiments where they are more clear, obvious and plain; ſuch as *cannot* be miſtaken. Thoſe are, in paſſing the steam of water through iron and charcoal, which, they ſay, are proceſſes of the decompoſition of water. Now, reader, I refer thee to thoſe pages in this book where I have fully, and, I hope, ſatisfactorily treated them, and proved incontestibly that there is no decompoſition of water in theſe proceſſes;--that when zinc, iron, &c. are reduced by charcoal, in the proceſs they produce Mr. Cruikſhanks' gaſeous oxyde of carbon, wich is formed indiſputably of the fixed air and water from the calces, uniting themſelves to the fixed fire of the charcoal; and when it is made by iron, of the fixed air and water of the chalk uniting themſelves to the fixed fire of the iron. That the acid, generated by the burning of oxygen and hydrogen gaſſes, eſcapes with the electric fluid,--has been most unequivocally proved by the experiments of Sigr. VOLTA's pile: *(See my experiments and obſervations upon it.)* This fact I have clearly demonstrated by future experiments; ſo that I call upon the French chemists to diſpute it, if they can. Thus publicly I have thrown the gauntlet in their teeth; and certainly the public must look upon it as a fact, if this combination dare not contradict it.

Another erroneous fact, which formed another baſis to this French theory, was in paſſing the electric ſpark through the caustic alkali, oxygen gas, and azote; and Mr. Cavendiſh found the alkali impregnated with nitrous acid. This experiment, I have clearly and most unequivocally ſhewn to be from the acid that the electric ſpark carries along with it, being formed of an acid and fire. *(See my* VOLTA*'s Experiments.)*

Theſe, and ſome others, have been the baſis of their theory. All theſe leading facts I have, to the candid reader, ſufficiently explained in my last publication; and what I there advanced, my enemies have not attempted to contradict. But now their object is to quaſh all chemical enquiries and experiments. Perceiving the storm gathering on every ſide, they endeavour to *ſkulk*, and to lead the public from all further diſcuſſion --- that public they have ſo ſhamefully and meanly deceived*!

There is no ſaying to what great objects this French theory may lead us! Not only the making of diamonds, gold, &c. (the ornaments of life), but even food. For this carbon, in ſhort, inſinuates itſelf into every thing.—The ſtrongest food that I know of is the fine oil of olives, from the vegetable kingdom; and butter, from the animal kingdom: then let us analyze them according to the French principles. By burning 20 grains of fine olive oil, it turned a greater quantity of oxygen gas into fixed air than 20 grains of charcoal, and alſo produced conſiderably more heat, I found. Though we are ſenſible by paſſing through charcoal, the ſteam of water, a great quantity of fixed air is produced; therefore it poſſeſſes a quantity of fixed air—nevertheleſs, a larger quantity of oxygen gas is required in burning 20 grains of fine olive oil than 20 grains of charcoal, and alſo a larger proportion of fixed air is produced. The oil containing ſo much more of fixed fire, or combuſtible matter; therefore producing ſo much more heat, and conſuming, in the ſame proportion, more oxygen gas. So that Mr. Lavoiſier, in his *Elements of Chemiſtry*, (page 106.), is obliged to ſay, "The ſum of theſe two gives 138.33776 "pounds of ice, which the two conſtituent elements of "the oil would have melted, had they ſeparately ſuffered "combuſtion; whereas the oil really melted 148.88330 "pounds, which gives an exceſs of 10 54554 in the re"ſult of the experiment, above the calculated reſult,— "from *data* furniſhed by former experiments."

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* Sequin, a great French chemiſt, who was one of the Committee with Lavoiſier in firing oxygen and hydrogen gaſſes, and which they ſay produced water. This great French chemiſt then, not ſatisfied with this wonderful theory in producing water, muſt give this ſyſtem activity by applying it to the human ſyſtem. And hear, O ye gaping profeſſors and phyſicians! Attend to theſe Oracles of Science! Gape wide, and ſwallow all — for it muſt not be a common gulp, but a *gorging* one . . . . *Gelatine is now found to be the great febrifuge principle!* —— Cram your patients with it; then a fig for the plague and yellow fever! The only diſcovery in medicine that this *aerial* theory could boaſt of, was, that *that* little conjurer, OXYGEN, is a cure for the Itch, particularly when ſnugly ſeated in Manganeſe. But now the leap is from the Itch to Fever — a more ſerious, but, perhaps, not a more general complaint. I wiſh they could find out a CURE for the SPIRIT of groſs MISREPRESENTATION, of *beating down* TRUTH, and *ſupporting*, in its ſtead, the moſt deſpicable PUERILITIES; and cure *that* ITCH for DEFAMING the greateſt and moſt important DISCOVERIES, and raiſing up a worthleſs French theory in its ſtead! And this done at the expence of true Science, Honour, Juſtice, Humanity, Common Senſe and Reaſon! When our children ſhall ſee this conduct in its true light, I have often wondered what they will do with the authors of it? *Take them out of the grave, and whip them.* Nay, nay, that will avail nothing; for if they had no ſenſibility of propriety when living, what can be expected from them when dead?

As oil therefore is ſo nouriſhing a ſubſtance, I do not ſee why charcoal ſhould not be alſo the moſt nouriſhing food; nay, particularly diamonds, as they are pure charcoal, not united to any other chemical body, and therefore more eaſily formed by the animal organs into nouriſhment: *I ſay, if their principles are true, it ought to be ſo.*

As I have obſerved, it is ſaid that Sir Joſeph Banks has obtained fame by counting his fingers: but I again wiſh many who have 'obtained fame,' had got it as innocently. I expect that poſterity will accurately ſcrutinize the conduct of *ſome;* and inſtead of fame, DISHONOUR will be their lot, *as the real enemies of true ſcience and merit.*

I am aware of the intricacy of the ſubject, and the great attention required in fathoming and elucidating the profundity of the phenomena. But after I had done that, and given them a theory which fully elucidates all the phenomena, upon the moſt rational and juſt principles—not to acknowledge it! but by every baſe art and chicanery to ſuppreſs its inveſtigation, that the world might not be acquainted with it! To treat it with the utmoſt contempt, as though it were the moſt abſurd nonſenſe! Nay, to ſteal parts from it, without acknowledging their ſource! *What does ſuch treatment deſerve*, I again aſk the impartial world? Thus they have forced me upon this pointed and ſevere, ſarcaſtic and cenſorious way of writing, which (I hope) is repugnant to my diſpoſition; but am obliged to reſort to theſe uncourteous weapons, the more effectually to repel their ſhameful, illiberal, and unjuſt behaviour to me;—its example, I believe upon my honour, is not equalled in the hiſtory of literature: therefore I was compelled to take up this *unpleaſant foil* IN MY OWN DEFENCE, which I ſhall with the *greateſt pleaſure* throw down again, if they will come candidly, liberally, and honourably forward,—and give me an honeſt, fair, and public diſcuſſion. SCIENCE, TRUTH, HONOUR, and JUSTICE imperiouſly demand it; but theſe, alas! have hitherto called aloud in vain!

In Nicholſon's Journal, for *April*, Mr. Harrup has made an experiment ſimilar to ſome of mine which I publiſhed many years ago—That, in expoſing azote to the ſun in a glaſs veſſel over well water, the air became atmoſpherical,—indeed richer than the common atmoſphere! Impreſſed with the French theory, he is determined to account for it upon *its* principles: but we ſhall ſee with what ſucceſs. He ſuppoſes that the water is decompounded by the azote attracting the oxygen of the water decompounding it. But he is obliged to acknowledge, the azote receives a higher ſaturation of oxygen than atmoſpherical air. How happens it then that the atmoſphere is not all equal to this ſaturation, as in his experiments; as it has plenty of water to act upon, and as water is conſtantly from the principles of evaporation mixed with the atmoſphere—alſo from the water in the ocean, in rivers, and even upon the earth, *all* expoſed to the air! But this is a trifling objection, ſcarce deſerving to be mentioned; this is only ſwallowing a gnat! Then this azote muſt have ſo wonderful an attraction for oxygen as to decompound water, the hydrogen and oxygen having, as they teach, ſo ſtrong an attraction for each other. But this attraction appears very weak; for after the azote has over-ſaturated itſelf with oxygen, by mechanically agitating it in diſtilled water, or being expoſed to this water, the oxygen will be found in the water and the azote robbed of it. Moreover, we are taught to believe that azote and oxygen form the nitric acid; and if Common Senſe is to accompany us in theſe diſquiſitions, as the azote has ſo powerful an attraction for oxygen as to rob hydrogen of it, *even in this low temperature*,—it muſt be by chemical attraction; and if by chemical attraction they are united, they muſt form either nitrous air or nitric acid. But we have only as yet ſwallowed the *trunk* of the elephant—we muſt gulp on! Mr. Harrup is obliged to acknowledge the hydrogen has taken *French leave;* no traces of it can be diſcovered by him. There is a thin "pellucid whitiſh film" which lined the retort where the water was in contact; and if this is examined, it will be found *the earth which the fixed air ſo-luted;* and if he will examine his tea-kettle, he will find a ſimilar depoſit: *a well-known fact.* As the rays of the ſun ſaturated this fixed air into pure air, it depoſited this *earthy* ſubſtance.

To prove this clearly to Mr. Harrup, if he will add freſh azote to this ſame water, after it has undergone the firſt proceſs he will not be able to form it into pure air. But if it was merely the water being decompounded, the ſame water would do for the proceſs *ad infinitum*, for he will find diſtilled water will not do even for one proceſs! Now, Mr. Harrup, can you ſwallow the whole elephant, which the French theory muſt force you to?—But if you will conſult my different writings, you will ſee ſimilar experiments of mine, and *rationally* accounted for. Therefore, Mr. Harrup, leave the elephant to be ſwallowed by their deſpicable agents, who are willing to gulp any thing, at the expence of Common Senſe, Honour and Integrity,—which they are perfect ſtrangers to; they being the mere ſervile, mean, and mercenary agents, to *ſwallow* any thing, or *ſay* any thing, and to *force* the groſſeſt abſurdities upon the credulous public.

I have read different theories of our forefathers, but I never ſaw any ſo extremely abſurd and prepoſterous as a late theory grafted upon the abſurd doctrine of the compoſition of water, viz. in that ſimple proceſs the evaporation of water by the action of heat: the water is decompounded into oxygen and hydrogen, they ſay. And even that wily and artful defender of the French theory, Mr. Kirwan, I am told, *ſeriouſly* argues againſt this decompoſition upon evaporation. I can only ſay, *you wiſe men of Gotham,—you wiſe* aerial *philoſophers*, who ſupport ſuch an hypotheſis,—take a *gooſe*'s head, which pop into a veſſel of water; place them upon the fire; and while the head is cooking, hold *your own wiſe heads* over it, gaping wide enough to receive all the oxygen and hydrogen *ſteam* which proceeds from *your brother*'s head, and it will produce far more wonderful, ſurpriſing and aſtoniſhing effects, than the *credulous Doctor* (Beddoes and Davies) found from their '*breathing the gaſeous oxyde of azote!*' THEIR *ecſtatic*, *lunatic*, and *Laputatic* ſenſations will be nothing to YOUR's! Inſomuch, that each of you may boaſt—though not indeed in the language of Milton,

"Into the Heav'n of Heavens I have preſum'd,
"AN EARTHLY GUEST! and drawn empyreal air;"
BUT, 'Into the Earth of Earths I have preſum'd;
'AN AERIAL GUEST! by breathing grey-gooſe air;'

alias, our *learned philoſophers*' oxygen gas.—Take care, when it is well boiled, to eat the *whole head*, *fleſh*, *eyes* and *bones*, and particularly the *brains*, and then you will be thorough *aerial* chemiſts, accompliſhed in all *myſteries*; ready to aſſert any thing, or ſwallow the elephant at one prodigious gulp! And, *all for the honour of the beginning of the nineteenth century!* In the courſe of time, (perhaps before this famous century is half finiſhed), they will be able to ſwallow St. Paul's cathedral:—they have already ſet fire to the water of the Thames.

Count Rumford, in his truly abſurd flights upon his *ſuppoſed* diſcovery of the non-conducting power of water, after ſuppoſing it accounts for animal heat, the ſolution of bodies in liquids, and a hundred more *Quixotiſms*,—ſays, that when hot iron is plunged into water no ſteam would come from it, if it was not owing to the decompoſition of the water. Good Heavens! *are we* entirely *to loſe ſight of Common Senſe in this* AERIAL *age?* Let but a friend of this diſtracted philoſopher plunge a red hot ſtone (or any other body) into water, will it not produce ſteam? And let the aforeſaid wonderful philoſopher *ſee* the experiment, and caution him againſt his abſurd whims, even though the Royal Society ſhould adjudge his own medal to him!

(See his ſeventh Eſſay, part 2d, page 361.)

If a child were to advance ſuch opinions, and perſiſt in them after ſhewing him the experiment; if he was ſtill not to be weaned from his vain conceits, I ſhould apply his *head* to the *hot ſtone;* and you know, Mr. Cavendiſh, "*a burnt child dreads the fire.*"

But permit a humble philoſopher (who has no *aerial French flights,* but travels peaceably on with *old* Engliſh Common Senſe and Reaſon, thoſe *old*-faſhioned folks) to reſtrain theſe wonderful diſcoveries, and to cry, '*Hold, hold!*' for fear, as you have already ſet fire to the Thames, you may next ſet the Ocean on fire! But indeed, if in your *ſapience* you can reſtrain the combuſtion—if you can (to imitate the ſtyle of a certain poet)

Sail on the burning wave and guide the flame,

why, then it would be an excellent plan to ſet fire to the enemy's gun-boats. What think you of this ſcheme, *ye wonder-, wonder-, wonder-*FUL, wonder-WORKING philoſophers! Ye that have pointed out the tranſmutation of charcoal into diamonds, of iron poker and tongs into gold, of water and charcoal into *good French brandy,* but I am afraid *not* into *GOOD BRITISH SPIRITS.* Nay indeed, far more amazing diſcoveries ye have effected; and as ye have already accompliſhed the principal point, (viz. the ſetting fire to water) it is only directing the courſe of the combuſtion. And as ſome of your wonderful body, (namely, Mr. Kirwan and Dr. Darwen) have planned and ſuggeſted the direction of the winds, keeping *Old BOREAS* in order; why not *Old NEPTUNE* in order?——*In ſhort, what is it that you cannot perform?*

——But I am afraid the *old boy*, when he finds his houſe (or element) on fire, will turn a little obſtreperous. If ſo, to pacify him, get Dr. Beddoes and Mr. Davies to make him drunk with *the gaſeous oxyde of azote**. But probably it will not be neceſſary, as the great conflagration and combuſtion will produce a conſiderable evaporation, alias *decompoſition of water.* Therefore, *Mr. Neptune* muſt breathe an immenſe quantity of oxygen, and, in conſequence, an immenſe excitement, alias *dead drunkenneſs*, muſt enſue†. Bravo! bravo! *ye ſurpriſing* aerial

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* Count Rumford, you breathed this gas at the Inſtitute, and therefore know its effects. But, you cunning rogue! I am pretty certain you had breathed it long before; as your important diſcovery that water will not tranſmit heat,—and the great, wonderful, and ſurpriſing theories you have produced from it,—clearly evince that ſuch wonderful flights could only reſult from the Laputatic effects of this gas.

† Reader, thou wilt marvel that this SOURING principle, (oxygen), ſhould produce *drunkenneſs;* but theſe paradoxes in their doctrine are nothing! Oxygen is a French conjurer, that can take all ſhapes and qualities. In ſhort, he is the *hocus pocus* of their philoſophy; therefore thou muſt wonder at nothing, or elſe thou art not a *true* diſciple of this French philoſophy, thou art not orthodox, *nor qualified to become an* aerial *conjurer thyſelf.* For Reaſon and Common Senſe thou muſt totally diſcard: but, gape wide and ſwallow all! St. Paul's, if it be neceſſary — or even the new Edinburgh Pharmacopæia. Gulp all! CRAM *is the order of the day!* For deſperation has ſeized them, and the *credulous* public has hitherto favoured them. But the day of retribution *muſt* and *ſhall* come. Then woe be to them; for, upon my honour, I would not have acted the part they have done to me, *even for all my diſcoveries.*

Alſo thou wilt wonder that *that* ſimple proceſs Evaporation, ſhould be a decompoſing proceſs; decompound the water into oxygen and hydrogen. But ſuch it is ſuppoſed by our *aerial* French theoriſts. It is ſo generally *philoſophers!* This will be giving the French a *Rowland for their Oliver*,—turning, with a vengeance, their own philoſophy upon them.——Oh, what our *cunning little SOLOMONs* can accompliſh! Exalt this age above all others! If Swift had been now living, he would have ſeen his *Laputa* outdone. They have even outrivalled the famous Knight of *La Mancha;* THEIR *aerial* flights far exceeds *his!* Mr. *Monthly Reviewer*, as you have in your *great penetration*, alias *jeſuitical low cunning*, endeavoured to inſinuate that my *old Engliſh reaſoning* againſt our French *aerial* philoſophers, is all "*modern waggery*," I will tell you, in return, that *your's* is all *modern knavery*.

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received, that Mr. Kirwan ſeriouſly endeavours to refute it. But thou muſt wonder at nothing in this very learned age. CRAM ALL is the order of the day: the more marvellous, (*alias* abſurd) the better.

In the French Dictionary of Natural Hiſtory, publiſhed by all the great Maſters of this *aerial* Philoſophy—CHAPTAL, PATRIN, &c. &c. The latter believes in this groſs, puerile and abſurd idea, that "the evaporation is owing to the decompoſition of water." The APPENDIX to the laſt Critical Review, ſays, "We have already ſpoken in commendation of "this article on the Atmoſphere, but forgot to obſerve that the author "ſtrongly ſupports the opinion frequently introduced into this Journal, "that evaporation is owing to the decompoſition of water, and rain to its "reformation of elements."——Upon my honour I am at a loſs whether ſeriouſly to argue againſt ſuch contemptible puerilities, or not;—I am afraid that my *rational reader* will think me as abſurd as them. Pray, are ſpirits of wine, æther, or the volatile oils, decompounded by evaporation? But collect the water in a ſtill, and no oxygen or hydrogen appears, nor is there any combuſtion upon "its reformation of elements." Heavens! what firing and exploſions we ſhould have in the clouds upon the formation of rain! Hold a piece of cold glaſs where water is evaporating, and it is immediately covered with water, which will trickle down its ſides as water. Put the TOM FOOL, the Reviewer, into a culinary veſſel that cooks victuals by ſteam, and boil, *not* roaſt him, till he has acquired a little common ſenſe—What a taſk have I, in expoſing ſuch groſs abſurdities!

You ſay, I am a *wrong-headed chemiſt:* but I aſſert, (and I think with a little more juſtice than you do), that you are a *wrong-hearted critic,*—and every fool is capable of being that. Theſe Reviewers think they are fighting *maſked;* but in a little time I ſhall undeceive them, and drag the recreants into open day!—They ſay, "It muſt be confeſſed that the preſent author *(me)* poſſeſſes facetiouſneſs in an uncommon and ſupreme degree:" Allow me, then, to indulge this vein a little; and, in recompence, I will condeſcend to introduce ſuch *low characters* as *even you* into the DRAMA. Now, as our French philoſophers have for theſe twenty years been ardently purſuing their Quixotic atchievements of attacking windmills, &c. their renowned philoſophical knights have reached the remoteſt corner of the globe, and petrified the *gaping* credulous multitude with their wonderful exploits, outſtripping all philoſophical knights that have gone before them. They have arrived at the ſummit of their fame, at that period when men commonly take poſſeſſion of their rewards for their aſtoniſhing actions upon this globe: juſt at that preciſe period of hiſtory when the valorous knight of La Mancha and his faithful 'ſquire take an *aerial* voyage upon the famous flying horſe called PEGASUS. Now, Reader, ſuppoſe this famous Pegaſus properly bridled, ſaddled, &c. and the French chemiſts perſonating the renowned knight, and our Engliſh philoſophers, (the ſervile humble followers), the *honourable* 'ſquire, Sancho Panca. Then ſuppoſe them mounted, and the *Peg* turned:—they take their *aerial* flight amidſt the ſhouts of the bawling and clamorous Reviewers, *and others;* as I certainly muſt do the mercenary hacks of literature the honour of introducing them into the DRAMA; and this they muſt not be angry at, as I mean to act a part myſelf—my office is, to hold the torch, to *enlighten the ſcientific world*, and (at the ſame time) to *ſinge* the *beards* of theſe wonderful *aerial* voyagers when they ſuppoſe themſelves approaching the ſun. Now they ſit exulting that they are performing deeds that the page of hiſtory was never bleſſed with — only they begin to make a few wry faces, and ſhuffle a little when *ſcorched* by the fire, kicking each other's ſhins. Not half ſo *cool* and intrepid as the knight, but partaking more of the agitations, diſtortions, and *certain motions* that poor Sancho was ſo unfortunately ſubject to, when fear got poſſeſſion of him. Nay, it has been whiſpered that even you Reviewers were ſo very unpolite, when you thought you were not obſerved, as to laugh and ſneer contemptuouſly at your maſters. — But when Pegaſus was ſuppoſed to deſcend, and the philoſophers to diſmount, having accompliſhed their voyage; and when the bandage was removed from their eyes, you abjectly fell proſtrate on the ground, (a part which you *acted* with admirable propriety) diſplaying all the fawning ſervility of the ſpaniel! Such a wonderful, aſtoniſhing, and ſurpriſing exhibition, the world never ſaw before, and, in all probability, will never ſee again. —— But no ſooner were our philoſophers ſafely diſmounted, (without broken bones indeed, but in a ſad *perturbation* and *ſituation)*, I collected all their books of *aerial* chemiſtry, &c. which had turned their poor weak brains, and inſtead of burning them,—(the fate of the poor knight's library)—I tied them upon the back of Pegaſus, and, turning* the

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* I was anxious alſo to have got hold of thoſe peſts to true ſcience, the Reviewers, to have ſent them this Lunatic voyage; but theſe cunning book-traders, thoſe "ſnakes in the graſs," eluded my ſearch.

Peg, directed his head to the *Moon*, judging they would travel thither, in return for the ſtones and rubbiſh which the *Luna*rians (according to the *profound* French philoſophers, De la Place and others) have ſent down to our earth*: at the ſame time fixing a moſt cavalier note in Pegaſus' mouth, deſiring no more intercourſe between the aerial moon and our ſolid earth, to bewilder the poor weak brains of its terreſtrial inhabitants.

In the year 1785 I publiſhed the proceſs which Nature takes in forming and renewing the atmoſphere from the great injuries it muſt ſuſtain from the reſpiration, the vegetation, and the putrefaction of all bodies, viz. by the ſaturation of the rays of the ſun with the fixed air and water, they meet with in the higheſt part of the atmoſphere, as when air is injured, it riſes to the higheſt part of the atmoſphere: a well-known fact. In attending the late Dr. Hunter's as alſo Dr. Monro's anatomical theatres in cold weather, when they were filled with ſtudents, I remember that the water from their breath was condenſed at the window in the middle of the ſuperior roof of the building, and would fall by drops upon thoſe who ſat immediately underneath. And from the experiments of Mr. Saſſure upon the higheſt mountains, he found an amazing proportion of fixed air in the atmoſphere, turning lime-water immediately turbid. I think I need not here mention the trifling and abſurd ſuppoſitions of the air being renewed by vegetation and water. For the former, I have proved clearly, injures the air; and as to water, *that* which is in our rivers, when it firſt flows from the ſprings, is clearly impregnated with fixed air: but by being expoſed in its courſe to the rays of the ſun, it becomes impregnated with pure air and azote. Any one who has examined the air of rivers and ſprings, will be ſenſible of this.

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* Will it be believed, my candid *ſolid* reader, that ſuch abſurdities are ſeriouſly advanced by our *aerial* French philoſophers as the immortal diſcoveries of the preſent day! this wonderful age! this age of revolutions!—*O tempora, O philoſophia!* O cunning SOLOMONS!

Now as the air is renewed in the higheſt part of the atmoſphere, what comes then, according to their theory, of the carbon, which muſt (in conſequence) be ſeparated from the fixed air upon its turning into oxygen gas.— Have the French philoſophers as yet diſcovered the ſhowers of coal which muſt, in conſequence, fall from the higher part of the atmoſphere; and they need not have recourſe to that wonderful flight of French imagination, to ſuppoſe *they fell from the moon?* But in Nature's elaboratory, in ſeparating the oxygen and carbon, ſhe muſt have ſome *ſly* method of ſecreting the carbon, which has not yet been diſcovered, (—I dare ſay Mr. De la Place can *find it out*); but we have as yet diſcovered no ſuch coal-mine in the clouds, if I may ſo expreſs it. *But there muſt be*, according to the French philoſophy: IT cannot err, our *learned* Engliſh chemiſts ſay. As this is an age of ſpeculation, what if the *learned combination* was to raiſe a fund for ſearching for it? I do not know but it would be a better ſpeculation than turning charcoal into diamonds, as by that means they would be ſo common! Or turning into gold the metals which our tongs and kitchen utenſils are made of;—it would be alſo common. But as coal is a neceſſary of life, therefore it would always have a great demand.

But, to advert to that *bright* idea of Mr. de Lucs, that *water is decompounded in the clouds*,—which idea he adopted, after I had ſhewn that the clouds are Nature's Elaboratory in renewing the atmoſphere, (though he never once mentions my name). Now as this idea deſtroys their coal-mine, yet it may ſtill be aſked, What comes of all the fixed air which is conſtantly forming in the atmoſphere from animal reſpiration, &c. &c.? and alſo, What comes of all the hydrogen, which muſt alſo be formed from the decompoſition of the water in the clouds?——To ſolve theſe difficulties, the French aſtronomers muſt lend their aſſiſtance, and ſend them both to the *moon*:—Thus, by means of the French chemiſts and aſtronomers, together with their auxiliary the *moon*, our gaping, credulous Engliſh philoſophers, can never be at a loſs in accounting for all the phenomena.—What a glorious age is this for philoſophy! Mr. Cavendiſh and Mr. Kirwan! happy your lot to live in ſuch an one.

It is a well-known fact, that injured air becomes lighter, and flies to the uppermoſt region of the atmoſphere. If my reader wants an experiment, he ſhall have an old one, which ought not to be deſpiſed on that account.—Let him take two birds, and place them in a cloſe cylindrical veſſel, and he will find that the lower one will live longer than that which has the loftier ſituation, *from this cauſe*. Now, in the lowermoſt part of the atmoſphere, there is not an atom of fixed air: Mr. Woodhouſe ſays, it contains not a thouſandth part. But *let us not look to our* obſcure experiments *in the laboratory*. No; for in theſe minute and delicate experiments, man is a poor weak bigotted being. *Let us attend to* Nature.—If fixed air were to be found in the lower part of the atmoſphere, it muſt in conſequence be imbibed by the waters that cover the globe; as water and fixed air have ſo great an attraction for each other! It is a certain fact, that Mr. Davy, in one of his experiments, inſtead of the fixed air which he expected, finding only oxygen gas, ſays that the water imbibed it, and it expelled the water's oxygen gas! But in this I proved that he was miſtaken, that the water (in this experiment) poſſeſſed *no* fixed air.——But I mention this to ſhew how impreſſed they are, *and juſtly*, with the great attraction water has for fixed air. But then in what way do they account for theſe two inſeparable facts,—That rivers iſſuing immediately from their ſprings, are highly incorporated with fixed air; but that, by being long expoſed *in their courſe* to the action of the ſun, this fixed air becomes oxygen gas? Now this ſufficiently demonſtrates that there is no fixed air in the lower part of the atmoſphere.—By cauſtic earth and ſalts attracting fixed air, I have clearly proved that it is the fire, and the earth and alkaline ſalts decompounding or ſetting looſe, by burning the fixed fire of the oxygen gas, leaving its fixed air as the reſiduum. But theſe cauſtic bodies will actually decompound both ſulphur and phoſphorus of their fixed fire, as I have fully ſhewn in my former writings. From this Mr. Barthollet plainly deduced, that ſulphurated hydrogen gas, by being made from lime, had many properties of acids, and alſo when united to lime.

In the Philoſophical Magazine for January 1803, I ſee that Mr. Phillips has called in queſtion the great Bergman's analyſis of carbonated lime, who made it conſiſt of Carbonic acid, 34—Lime, 55—Water, 11. Experiments made by thoſe unacquainted with the true principles of chemiſtry, are generally erroneous:—But every novice attempts it, eſpecially ſuch as are wedded to the French theory. Kirwan and others, in aſcertaining the water which different chemical bodies poſſeſs, content themſelves with ſuppoſing it to be the water of compoſition, not conſidering that water alſo joins the acid in neutralizing the earth, ſalts, &c. But without inveſtigating minutely the experiments of Kirwan, &c. I ſhall at preſent only throw out a few general remarks, reſerving to myſelf the right of entering upon a more rigid ſcrutiny at ſome future opportunity.—Water enters into the compoſition of theſe bodies in two ſtates:—firſt, entering with the acid; and then, ſecondly, forming the water of compoſition. This is ſtrikingly exemplified in taking pure lime and expoſing it to the carbonic acid, which it will not attract; yet, by adding water, they both immediately chemically unite to the lime, forming carbonated lime: but the quantity of water required is very conſiderable. They unite with the lime in its mild ſtate, expelling its fire; and after that, more water is required to form its water of compoſition. By this manner of conducting the experiment, I have found a greater quantity of water enter the lime than fifteen per cent. whereas Mr. Phillips makes it only about one-half per cent.! I ſhall only requeſt that gentleman to perform the experiment in my way.

But, reader, haſt thou never noticed the ſimple operation of *a man ſlaking lime*—what an immenſe quantity of water is chemically attracted! and this water only forms it into a dry powder: it is ſtill in part lime, wanting its carbonic acid. Beſides, the quantity of water that the carbonic gas contains is conſiderable, as Barthollet obſerves. Now here are their favourite *unerring* French chemiſts acknowledging it! The ſtrong concentrated acids, like the aerial acids, will not unite to many bodies, metals, earths, &c. unleſs they are mixed with water, juſt upon the ſame principle: And this water enters the body the ſame as the acid, a part thereof going to the aerial compoſition of the gas,—as hydrogen and nitrous gaſſes, &c. Therefore, employing the concentrated mineral acids, they ſeize the water of the carbonated lime, and enter with it, forming the ſelenite. For if it was not from this cauſe, how comes it that the mineral acids require to be diluted in calcining metals, &c. as the nitric acid. The water, they ſay, is not the calcining body here, as it is in the other mineral-acid calcinations; but, take the ſtrongeſt concentrated acids, and apply them to the metals, and no calcination enſues. Add a certain proportion of water to them, and an active calcination takes place. Then accurately examine the ſalt formed, and the water has perfectly diſappeared; gone to the formation of the ſalt and the gas:—preciſely the ſame experiment juſt named, of the carbonic acid and lime. It is preſumed that this is a ſufficient explanation HOW the water acts in theſe proceſſes; for, on any other, I again aſk WHY it is required to be mixed with the nitric acid in theſe calcinations?—It ought to be of *no uſe*, according to *their* theory; but in *mine*, how beautifully do all theſe experiments agree! The fire of the lime and the fire of the metals can only be ſeparated from them by the joint chemical action of both the acid and water, and in one caſe the fire is ſet looſe in a free ſtate, and in the other, in a fixed ſtate. Now every chemiſt muſt ſee the fallacy of their experiments upon the compoſition of ſalts.—But I intend to give a complete HISTORY of theſe BODIES, when I can diſcover a diſpoſition among my contemporaries to liſten to the voice of REASON, TRUTH and COMMON SENSE.

There has been a deal of conjecture about the phoſphoreſcence of wood, the elucidation of which has always been imperfect. Wood, like all other vegetable and animal bodies, undergoes putrefaction,—and putrefaction conſiſts in the body decompounding atmoſpherical air by attracting fixed fire from it, ſo that an animal body ſhall become (in a manner) all *ammonia* by it; and vegetables alſo, in their putrefaction, attract an immenſe quantity of fixed fire from the atmoſphere. Wood is a vegetable body that undergoes putrefaction very ſlowly; and when it arrives at its ultimate ſtate of putrefaction, it there ſtops. But as Nature exiſts by *compoſitions* and *decompoſitions*, and as nothing is ſtationary, there is *a ſtate* when this immenſe accumulation of fixed fire tends to diſſolution. In animal ſubſtances this fixed fire is principally diſſipated in its fixed ſtate, as putrid effluvia; alſo the ſame in the *ſoft* vegetable ſubſtances: but in the *hard* wood a ſeparation of its fire ſometimes eſcapes as *free fire* and *light*. Thus in WOOD, all its reſinous parts, (or parts of its fixed fire), are broken down, and adhere to its fibrous parts, uniting in a looſe way,—ſo that when moiſture and atmoſpheric air are applied to it, a combuſtion enſues. The water acts by attracting the fibrous part, and aiding the expulſion or ſeparation of the fire. *(Oil will do the ſame.)* It is ſo looſe, as Monſ. Caradori found, that phoſphoreſcent wood, if moiſtened, will even *ſhine* in a *torricellian vacuum* for a ſhort time. But air is in general neceſſary to its illumination, as Mr. Hulme found.—*(See the* Philoſophical Tranſactions.)

Phoſphorus is a body which alſo burns in air, ſpontaneouſly. This implies that it is very combuſtible; and that it acts upon the air by injuring it, is well aſcertained *by all their experiments.* There are various phoſphoreſcent bodies, all of which act equally upon the air, ***injuring it:*** but this they do in a *ſlow* and ***gradual*** manner: but if great heat be applied to them, they burn and conſume *rapidly;* and if a great degree of cold, it checks their combuſtion: from whence ariſes this conſequence, that *a certain degree* of heat is neceſſary? All this Mr. Hulme found. But that it is an *active*, though *ſlow* combuſtion; that it takes place in the air without a high temperature of heat, are indiſputable facts. It is found alſo, that they do not require air of a high ſaturation of fire,—for they burn in azote, even better than in artificial oxygen gas, (as Mr. Getling diſcovered), and which produced a great controverſy. But in none of the other gaſſes will they burn, as the experiments of Hulme and Carradori demonſtrate. In artificial oxygen gas, its fire being more ſtrongly attracted by the acid, it is not ſo ſoon let looſe; therefore it is not ſo favourable to the burning of phoſphoreſcent bodies as atmoſpherical air. It is the ſame in reſpiration. Animals cannot perfectly injure this air. *(See my letter to Mr. Cavendiſh, page 26.)* So that Dr. Prieſtley is conſtrained to ſay, (vol. 5.), "I found that mice would not live in dephlogiſticated air, *(oxygen gas)*, 'till they had completely phlogiſticated it, though they lived longer in it than in proportion to its purity, with reſpect to common air; and for this I cannot aſſign any ſufficient reaſon."

But that phoſphoreſcent bodies produce ſo active combuſtion, that they will even act on atmoſpheric air, that a candle has been extinguiſhed in it, or that animals have died in it, or been *injured* by any other proceſs—that it contained nothing but the pureſt azote,—are well-eſtabliſhed facts. Yet as Hulme, Carradori, and others, found that the phoſphoreſcent bodies burned in it extremely well, it is from hence evident that they were ſtill capable of acting upon this air, though it was not ſo highly ſaturated with fire. This clearly ſhews that it is a very *active* combuſtion, though *ſlow*. The heat produced by it being ſo very low, and burning in ſo low a temperature, are circumſtances which occaſion the proceſs to be *very ſlow*—ſlowly ſetting looſe its own fire, as well as the airs. But if the temperature is raiſed, it becomes very active, as Mr. Hulme found. Alſo it acts very ſlowly in injuring the air; for when the fire was extinguiſhed in the phoſphoreſcent bodies, by adding freſh bodies the air would ſtill ſupport their combuſtion. —Mr. Hulme's experiments plainly ſhew, that *fiſh*, in order to their becoming luminous, ſhould for ſome time be expoſed to the atmoſphere; for, when covered, no illumination took place. Then, upon expoſure to the air, the fiſh attracted its fixed fire from it, injuring the air. Therefore, from receiving a ſaturation of it, it then is capable of burning ſpontaneouſly in it. But if the fiſh runs into the putrid fermentation, the fixed fire it receives from the air, becomes *ſo fixed*, that it will neither burn ſpontaneouſly, nor emit the phoſphorent light.—— For which reaſon, bodies becoming phoſphorent, require that they ſhould poſſeſs fire in a looſe diſengaged ſtate. *That in* FISH, their phoſphorence takes place before putrefaction: *that in* WOOD, putrefaction muſt have ceaſed, and the fixed fire muſt be in that looſe ſtate, ſo as to be ready to ſeparate from the wood.

The diſſertation of Thenard's, upon the vinous fermentation, envelopes this wonderful French theory moſt ſtrikingly; and, Reader! if thou art even capable of ſwallowing the whale, it muſt certainly ſtagger thee.—The vegetable juices of FRUITS, are ſuſceptible of the vinous fermentation;—from the ſaccharine ſtate, they enter into the vinous,—foregoing the ſaccharine quality, and acquiring the ſpirituous. He thinks he has diſcovered the principle of fermentation, and calls it an animal glutinous matter, which converts the ſugar into alcohol and the carbonic acid. But this glutinous matter only could act upon ſix times its weight of ſugar, therefore he thinks it is not the pure ferment. This ferment produced by diſtillation, ammonia; but after it had undergone the proceſs of fermentation with ſugar, it would produce no volatile alkali. In ſearching for this *animal matter* before fermentation, he could not detect it by all the re-agents which chemiſtry affords him, but ſucceeded by fermentation; and it afforded him a great quantity of ammonia, by diſtillation, and other proceſſes. He is rationally ſtaggered in ſuppoſing this yeaſt to be excluſively employed by Nature as the ferment, for this clear and obvious fact, Reader!—*It is depoſited after the fermentation has taken place.* But then, as it appears neceſſary to his theory, IT MUST BE SO. This matter of ferment ſhews the preſence of azote by the nitric acid. He ſays, "There is evolved from it, at firſt, azote mingled with carbonic acid; and afterwards nitrous gas, at the ſame time."—He found that the ſugar and the ferment were principally formed by fermentation into the carbonic acid and alcohol, and the reſiduum of a little acetous acid, *but no azote*, by the moſt accurate examination that he was maſter of, and not even by the moſt diligent ſearch could he detect it. By ſome legerdemain work, this *French conjurer* had diſappeared. But as this element is ſo neceſſary to *French theories*, he examined vegetables by various other proceſſes, —*but no ammonia*. However, he gravely tells the Inſtitute, that he hopes to find this little truant by future experiments. But at the concluſion of this *ſelf-important paper*, he is obliged to ſay, "I know not whether I ſhall be able to diſcover "what becomes of the azote of the ferment."——But, Reader! ſuffer me to propoſe to thy Common Senſe a little queſtion: —As this ſame azote in vegetables is the great animalizer, ought it not to be in the greateſt abundance? Conſider what a quantity the Ox muſt receive! Examine his evacuations. What immenſe quantities he is regularly diſcharging, as noxious to his ſyſtem! Then ſurvey his ſtupendous ſize! his immenſely huge carcaſe! After that, if thou art not convinced, I will ſay, thou oughteſt to be *diſciplined* like a ſchool-boy; let *ſtripes* be the cure of thy ſtupidity! Thou mayeſt then ſwallow Oxen, Elephants, Whales; nay, even St. Paul's! In that caſe, thou haſt a *ſtomach* for *any thing*, and a *head* for *nothing!* CRAM is the order of the day!

But, to come cloſer to this *immortal* theory.—He ſays, he believes with Lavoiſier, that "each particle of ſugar is formed—eight parts of oxygen, four of carbon, and one of hydrogen." Now, Reader, let us ſee how the ſugar is formed, neceſſary for fermentation. Barley is what we principally form into ſpirits. In order to do this, we malt it *.

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* Mr. Cruikſhanks ſays, (*vide* Nicholſon's Journal, page 338.), "To be more certain of the nature of the change which the pure air undergoes in this proceſs, the experiment was repeated as follows:——*January* 23.—A quantity of barley, ſoaked in water for two days, was introduced into a jar containing 46 meaſures of very pure oxygen gas, and inverted

Now, clearly, in order to this grain forming its ſugar ſtate, it muſt form oxygen into the aerial acid. Here, then, is a proceſs of forming the carbonic acid, inſtead of oxygen, when ſugar is formed.—I am really aſhamed of ſpending my time in expoſing ſuch childiſh abſurdities. Know, Reader! (if thou haſt a grain of ſenſe to receive it) that ſugar is a neutral ſalt, formed of an acid and fixed fire, and that the malting proceſs acts by receiving the fixed fire of the oxygen gas into the ſubſtance of the barley;—that when this barley is macerated in water, it ſtill attracts more fire from the pure air of the atmoſphere, producing fermentation; and if it is diſtilled it will attract part of the fire neceſſary to its evaporation or diſtillation, conſolidating or fixing it into its ſubſtance, forming *alcohol.*

Having ſhewn the formation of alcohol by ſyntheſis, let us ſee if we cannot do the ſame by analyſis.—Set looſe this fixed fire again by another proceſs of Nature—by the acetous fermentation. Mr. Thenard (page 42.) ſays, "If pure air be employed inſtead of common air,

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over mercury. At the end of three days the barley began to grow, and this proceſs continued for ten days, although very ſlowly: the column of gas remained exactly of the ſame height, ſo that it had undergone no apparent diminution or increaſe. The barley being withdrawn, the air in the jar was examined, and found to conſiſt of carbonic acid gas, mixed with only one-fiftieth of its bulk of oxygen gas. The barley was partly converted into malt, the quantity of oxygen being inſufficient to produce this change upon the whole."

Alſo Mr. Gough ſays, (*vide* Mancheſter Memoirs, page 491.), "It is evident that the putrefactive fermentation, or an emiſſion of gas from their ſubſtance, deſtroys the vegetative faculty of peas; from which we may venture to conclude that it has, ſooner or later, the ſame effect on all other ſeeds expoſed to its influence."

the reaction is ſtill more ſpeedy. I have introduced 15 *grammes* of ferment into a veſſel filled with a litre of pure air; I opened it over quickſilver; a fifth of its bulk was abſorbed; the ferment was grown ſour, all the oxygen gas had evidently diſappeared, and was converted into carbonic acid. The temperature was 15°."

Now this fermentation is likewiſe produced by attracting the fixed fire of the air, but there is a conſiderable internal heat kept up by the ferment, an inteſtine action, and alſo part of the oxygen gas is imbibed, turning not only its own acid to the acetous, but alſo the acid of the vegetable. Mr. Thenard could not detect any volatile alkali in vegetables, by diſtillation; but he could find ſome after the vinous fermentation, particularly in the yeaſt. Now the yeaſt is formed of that part of the vegetable which is moſt vegetized, and attracts the fixed fire of the pure air the moſt. Therefore, when applied as a ferment, the inteſtine motion being induced, partly ſets looſe its fixed fire, ſo that it poſſeſſes leſs ammonia, as Mr. Thenard found:—for ammonia is formed of fixed fire. Sixty *grammes* of it being employed by him as a ferment, he ſays, "There remained forty *grammes* of a ſubſtance, which I believed to be more animalized than the ferment itſelf. I was much ſurpriſed to find, that I obtained from it, by diſtillation, much leſs ammonia."—So, upon adding this to another fermentation, all the ammonia diſappeared, and he thought the azote was carried off with the fixed air; but he found it pure fixed air.—Then he concluded it muſt be contained in the alcohol; but there were no traces of it there: nor in the reſiduum could it be found even by the moſt painful ſearch!—Now, clearly, by the fermentation, the fixed fire of the ammonia went to the formation of the alcohol, and part of it might be ſet looſe in the fermentation; and therefore, clearly, ammonia is formed of fixed fire, and not of azote.—I have juſt given a curſory view of the proceſs of fermentation; probably hereafter I may enlarge upon that ſubject, when our theoriſts are willing to open their eyes to the clear noon-day.

Since writing the above on the gaſeous oxyde, I happened accidentally to light on a paper of Mr. Davy's, (in Nicholſon's Journal), expreſsly on this ſubject. As Dr. Beddoes and he had ſo exalted its wonderful operations upon Laputatic animals, he therefore honours it with a hiſtory, which I ſhall curſorily examine. He ſays, (page 283.) "Nitrous oxyde, perfectly pure, may be obtained in an eaſy manner, and at a cheap rate, from the decompoſition of nitrat of ammoniac. This ſalt, when deprived of as much of its water of cryſtallization as poſſible, becomes fluid as the temperature of 320 deg. Fahrenheit, and immediately begins to be converted into water and nitrous oxyde.

"One hundred grains of ſalt, perfectly decompoſed at any temperature between 320° and 620 deg. produces about 57 grains of gas, and 43 of fluid. In experiments on this decompoſition, the heat muſt be carefully applied and regulated; as at temperatures above 800 deg. nitrate of ammoniac detonates, and becomes changed into nitrous gas, nitric acid, water, and nitrogen."

Dr. Auſtin, who was not tied down ſo rigidly to the French theory, ſays, "Fixed air is alſo produced in this experiment." (As to the *votariſts*, I diſpute the accuracy of their experiments, on account of the temptation they are under to make them tally with their theory.)—

Now here is an immenſe quantity of water produced;—then how is it produced? Is it on the common principle of heat evaporating into vapour or gas, the nitrate of ammonia? Is there a decompoſition of its elements, and a freſh compoſition again,—ſo that the oxygen of the nitric acid unites to the hydrogen of the ammonia, ſo as to form water in ſuch an immenſe quantity, being almoſt half of the whole. But then, if that was the caſe, let us ſee, according to their *bright* theory, what would be the effect. When the nitric acid is decompounded in gunpowder, an immenſe exploſion takes place; and this exploſion, they ſay, is only from its oxygen attaching itſelf to the charcoal, which produces, *according to them*, by no means ſo much fire as when it unites to hydrogen. But in this proceſs the ſalt requires only a very low heat to form it into gas; for if the heat is conſiderable, it detonates. Then (if there is to be any common ſenſe in theſe diſquiſitions) whence comes the union of oxygen and hydrogen? This theory ſays, Detonation is the conſequence,—as in the firing of gunpowder and the eſſential oils, with the nitric acid. But here this union is to take place with no combuſtion. Yet if you raiſe the heat, a combuſtion *does* take place, they allow. And what is that combuſtion, according to them? Why, juſt the ſame proceſs the oxygen of the acid unites to the hydrogen of the ammonia *. Then, is not this wonderful, ye *ſervum*

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* In firing oxygen and hydrogen gaſſes, they ſay that theſe attractions are reverſed; that the azote which is mixed with that oxygen, unites to the oxygen, and forms the nitrous acid. The oxygen which is formed from nitre, mangane ſe or any other body, they ſay always poſſeſſes ſome azote. But it appears ſingular that theſe two airs ſhould be always formed in the ſame proceſs, and from the ſame body; they ſhould have united

pecus! that, in the direct ſame proceſs, in one caſe there ſhould be a combuſtion and detonation, and in the other *not*. How can you ſwallow this? But ſtill gape wider; *you have more to ſwallow*. The product in one caſe is the gaſeous oxyde, or what he pedantically calls *nitrous oxyde and water;* but, in the other, nitrous gas, nitric acid, water, and nitrogen. Heavens! what abſurdities! Now, rational reader, I will addreſs myſelf to thee. . . . The gaſeous oxyde is formed of identically the ſame bodies as the nitrate of ammoniac. The water is the water of compoſition and cryſtallization, which the ſalt requires, and which all ſalts poſſeſs. And when there is a combuſtion, the fixed fire of the ammonia is principally ſet free, leaving the nitric acid only partly phlogiſticated, as nitrous air, nitric acid, and nitrogen. Mr. Davy knows nothing of the water which ſalts require for their compoſition, their water of cryſtallization being only a ſmall part. He ſays, " When nitrous gas is expoſed to naſcent hydrogen, whether it be procured by the action of metals upon water, in common caſes, or in Galvanic experi-

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together in their naſcent ſtate, forming the nitric acid—as their theory teaches that bodies are more ſuſceptible of attracting one another, when in their naſcent ſtate. Beſides, if heat produced the combuſtion or union, we can apply a ſtronger heat to the manganeſe and nitre in their ſolid ſtate, when theſe airs are produced from them, than we can to airs —Alſo if they were both in the manganeſe, they muſt be chemically united.—— Therefore the manganeſe muſt contain either the nitric or nitrous acid, nitrous air, or the gaſeous oxyde of azote.'

But it is ſtill more extraordinary, the nitric acid in the nitrate of ammonia muſt leave its baſe (azote) to unite to the hydrogen, which is alſo fixed to its baſe azote, to form water But in the firing of hydrogen and oxygen gaſſes, when they are fixed to no baſe, the nitrous acid is formed. Meſſrs. SOLOMONs, I think this requires a little of your *hocus pocus.*

ments, ammoniac and nitrous oxyde are uniformly produced; and the volume of the nitrous gas diminiſhes nearly to the ſame extent, as in the proceſs with ſulphurated hydrogen, *i. e.* to about one-third."

The caſe where ammonia is formed is in an experiment of Dr. Prieſtley's, where he applied iron needles to nitrous air,—and the nitrous air was formed into the gaſeous oxyde; and by continuing the proceſs, this gaſeous oxyde became azote or nitrogen, and ammonia was *then* formed—*but not till then.* Now here the formation of the ammonia was when the calx attracted the nitrous oxyde's phlogiſton, according to my theory; but, according to their's, when it attracted its oxygen, as it was much reduced in volume, and formed into azote. (See page 110.) Doctors Prieſtley and Higgins experiments prove, that iron, expoſed to nitrous air, over mercury, when the air is reduced one-third, this gaſeous oxyde of azote is produced; and when two-thirds, azote.——Thus then all their experiments prove, that when the ammonia is formed, the nitrous air contracts in volume, loſing its inflammability, or detonating power; *turning into azote.* This is obviouſly the caſe. But here I ſuppoſe they will introduce that ABSURDITY OF ABSURDITIES, *the decompoſition of water*[*]. But let us ſee, in that

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[*] Alſo ammonia is formed, according to Humbolt and Vanquelin, by the ſulphet of iron attracting nitrous gas; alſo in ſhaking nitrous air with diſtilled water. Here evidently its formation is owing to the cauſes that I have aſſigned, and not to the decompoſition of water. But, that the phlogiſton of metals will form ammonia, we ſhall prove inconteſtibly when we come to treat upon Galvaniſm.

Mr. Barthollet ſays, that "on ſhaking nitrous gas with diſtilled water, nitrat of ammonia is formed."—*Appendix to the Monthly Review.*——Now, will my reader require me to go over again the ground which I caſe, what would take place: the hydrogen of the water would go to the formation of the ammonia, but what then becomes of this conjurer, *Oxygen* — not only what the gaſeous oxyde, but likewiſe the water contained — Here, *according to them*, the azote of the gaſeous oxyde ſhould have been attracted by the hydrogen forming the ammonia; and the oxygen of the gaſeous oxyde, and of the water, ſhould have appeared inſtead of the azote: ſo that there ſhould have been a great generation of oxygen, as, according to Mr. Davy and the Dutch chemiſts, the gaſeous oxyde conſiſts of about 37 of oxygen and 63 of azote. Then, when it was reduced one-half, as in Dr. Prieſtley and Higgins' experiments, it muſt have been by attracting its oxygen; but ſtill it would have left 13 parts of oxygen to the 37 of azote. And ſtill, what became of *all* the oxygen of the water, as the calcination of the iron takes place at firſt, when the nitrous air is turned to the gaſeous oxyde? For, after that, when the gaſeous oxyde is turned to azote, no freſh calcination is produced. Thus it is abſurdity upon abſurdity! For when

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have ſo recently trodden — ſhewing the abſurdity of its being from the decompoſition of water. Here is merely the mechanical action of the water, which ſeparates the rich phlogiſtic part of the nitrous air.—Can any thing be more clear and ſimple?

Dr. Prieſtley, when ſpeaking of this gaſeous oxyde of azote produced from the nitric acid and the oil of turpentine, ſays, "The air was produced very ſuddenly. I examined it immediately, and the air burned in it with an enlarged flame." (Vol. ii. page 128.)

There is ſuch a blind, headſtrong attachment to this French theory, that though common facts ſpeak ever ſo plainly, yet is the voice of reaſon drowned! The oxmuriatic acid is ſuppoſed to bleach linen by giving it oxygen; but does not this burning acid act ſo violently as to burn the linen?—Ye *Wiſeacres!* is this from giving it oxygen? Put your noſe into this acid, and it will be burnt. And is this alſo from giving it oxygen?——Go, ye Tom Fools, and learn your A, B, C.

the ammonia is formed, the azote ſhould have been attracted by the hydrogen to have formed it, and not the oxygen. But *their* formation of ammonia is a fallacy; for even the French chemiſts, in analizing it, could not find any azote in it, by all the various proceſſes they could invent.—Was there ever a poor devil of an author doomed to contend with ſuch groſs abſurdities as I have been! To drive out of the brains of our *wiſe philoſophers* ſuch ridiculous puerilities!

From the calx of iron attracting the phlogiſton of the gaſeous oxyde of azote, they therefore aſſert, as a general fact, that metals, when calcined by the nitrous acid producing nitrous air, ammonia is formed. But, ye ſenſeleſs theoriſts! will not ſulphur equally form the nitrous acid into nitrous air, and the vitriolic acid is the reſiduum? Then, Dr. Thomſon and Mr. Davy, what comes of your ammonia and light? Here is only the vitriolic acid and a ſupporter formed. And it is equally well known that the ſulphur will form the nitrous air into the gaſeous oxyde of azote, as well as metals. This you admit *. Muſt you not ſee the abſurdity of your reaſoning? It is truly ridiculous!

Thus it appears that ſulphur produces the phlogiſton to form the nitric acid into nitrous air, and the phlogiſton to form the nitrous air into the gaſeous oxyde of azote, as well as the metals.

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* See Dr. Prieſtley's 3d volume, ſection xiii. page 140. "Of the phenomena attending the diminution of nitrous air by iron filings and brimſtone, and alſo by liver of ſulphur producing the gaſeous oxyde of azote." ——See alſo Mr. Kirwan's paper in the Philoſophical Tranſactions, 76th volume, page 134. By mixing nitrous air and hepatic air, (that is, ſulphur aerialized), he formed the gaſeous oxyde of azote.

But the nitrous air, inſtead of attracting more phlogiſton from phoſphorus, ſets it on fire; and inſtead of the gaſeous oxyde of azote being formed, there is only the phoſphoric acid. Here, clearly, the nitrous acid which the gaſeous oxyde poſſeſſed, is turned into the phoſphoric acid: juſt the ſame as when the nitric acid is applied to ſulphur; part of it enters the ſulphur, forming the vitriolic acid; and the other part forms the nitrous air. And alſo, *equally as material a fact*, it proves that when the gaſeous oxyde of azote is not formed, there is actual combuſtion formed, and great heat and light; and that when there is no combuſtion and no heat and light formed, there is the gaſeous oxyde of azote formed.— Can any thing be more obvious, plain, and rational?— And let me tell thee, reader, if thou poſſeſſeſt the leaſt grain of common ſenſe, (" *though it be but as a grain of muſtard ſeed*"); and if, haply, thou art endowed but with the ſmalleſt portion of reaſon, candor and integrity, thou muſt aſſent to my doctrine; but if thou doſt not, thou haſt no pretenſions to theſe.

Phoſphorus forms the nitric acid into nitrous air, and the phoſphoric acid is formed; juſt the ſame as ſulphur does, forming, with the acid, nitrous air and the vitriolic acid. Moreover: The gaſeous oxyde of azote will burn both ſulphur *into* the vitriolic acid, and the phoſphorus *into* the phoſphoric acid: ſo that, on which ever ſide we examine it, it appears perfectly obvious and plain; and that our AERIAL *Conjurers* have no *key-hole* to eſcape out of.

I wonder what they will do; . . . . for I think it is impoſſible they can have the effrontery to continue their abſurdities, and they cannot perpetuate DECEPTION.—

There are only two ways open to them; candidly to acknowledge their error, — or endeavour by baſe artifice to ſuppreſs all further inveſtigation. And if they are obliged to *acknowledge*, it is an eaſy matter for them to get ſome of their body to make a few trifling experiments, and to pretend that they produced conviction. — *I believe them capable of any thing, however mean, diſhonourable and unjuſt.*

Now, *philoſophers*, learned and unlearned; *ſervum pecus; reviewers and journaliſts!* know the ſimple proceſs which nitrous air is formed by, viz. *the nitric acid, phlogiſton and water.* Know that this air, expoſed to freſh iron, the acid of the nitrous air, (as the acid ſtill predominates, it not being thoroughly neutralized), attacks the freſh iron's phlogiſton, and calcines it; a part of it entering the calx, and the other uniting itſelf to a full ſaturation of phlogiſton, changing, by that means, from ſour to ſweet. But, if it is ſtill expoſed to the calx, the calx will re-attract its phlogiſton from it yet, not ſo as to reduce itſelf, as the calx is united to nitrous acid and water, and there is no fire to expel them, nor to aid the calx to attract more cloſely the phlogiſton; but it is in a looſe ſtate, as ammonia: ſo the calx will be (if I may ſo expreſs it) *a nitrated ammoniated calx.* Mr. Davy ſays, that the Galvanic experiments will alſo form the nitrous air into the gaſeous oxyde of azote. Does not this then plainly indicate, beyond all doubt and ſuſpicion, that the Galvanic fluid contains phlogiſton? (For this abſurdity of the decompoſition of the water, equally applies to it.) Therefore, humane reader, thou wilt not ſurely require me to drag myſelf over the ſame ground, in contradicting that abſurdity of abſurdities, that error of errors, that puerility of puerilities,—*the decompoſition of water*[*].

Mr. Davy ſays, "During the action of zinc, iron or tin, upon diluted nitric acid, certain quantities of nitrous oxyde are produced, but mingled with nitrous gas and nitrogen. In theſe proceſſes, as has been long known, ammonia is formed."

Now, I directly ſay that *no* ammonia is formed.—What he alludes to (I ſuppoſe) is, the experiment of Dr. Prieſtley's with the nitrous air and the needles. But there is no ammonia formed in theſe firſt calcinations: ſee page 117, where the zinc, after its calcination, produced the pureſt oxygen gas, and no veſtige of ammonia.—— But we need not be ſurpriſed at theſe poſitive aſſertions, when we ſee what a groſs infatuation Mr. Davy is under. In fine, ſo long as men continue under the influence of ſuch a *mania*, it is impoſſible to reduce them to reaſon, unleſs they will correct themſelves.

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[*] Mr. Davy is one of thoſe philoſophers whoſe imaginary flights and credulity are equally ſhewn in his *ecſtatic ſenſations* upon his breathing the gaſeous oxyde. Educated under that vague theoriſt, Dr. Beddoes, and now placed in the Inſtitute, he muſt be influenced by his patrons. But I muſt candidly acknowledge that he diſplays great ingenuity in his experiments; and if he was not under the dominion of the preſent abſurd theory, and his genius properly applied, he might be an ornament to ſcience. But he muſt devote days and nights to ſtudy common ſenſe.—I have much more pleaſure in commending than in cenſuring—but unfortunately it has fallen to my lot to undertake a taſk more arduous than cleanſing the Argean Stable.

Mr. Davy muſt aſſuredly feel the impropriety of his neglecting to notice my Galvanic experiments and obſervations. It will be no excuſe for him to ſay that ſome of his *learned* patrons would not allow him.—Better d braved the fate of poor Dr. Garnet.

Mr. Davy (page 285.) ſays, “ During the combuſtion of ſolid and fluid bodies producing flame in nitrous oxyde, nitrous acid is formed, moſt likely in conſequence of a new arrangement of principles produced by the ignition of a part of the gas not in contact with the burning ſubſtance.”

Now, reader, ſeriouſly reflect upon their opinions. Here are inflammable bodies, which (they ſay) are formed of carbon and hydrogen; and here is an air, formed, as they ſay, of “ about 37 parts oxygen and 63 parts nitrogen.” Their theory moreover ſays, that ‘ combuſtion is conducted by oxygen attracting the combuſtible bodies, carbon and hydrogen forming carbonic acid air and water.’ But in this combuſtion he ſays nitrous acid is formed; a fact I proved many years ago; and here is no carbonic acid air formed. But, hear! *ye ſuperficial theoriſts; ye Tom Fools* of philoſophers! He ſays, “ This “ is moſt likely in conſequence of a new arrangement of “ principles, produced by the ignition of part of the “ gas not in contact with the burning ſubſtance.” What can be ſaid to ſuch a palpable abſurdity? If, Reader, thou canſt not ſee it, and laugh at it, I adviſe thee to vex thy pericranium no more with philoſophy: it was never formed for it. But, in lieu of theſe abſtruſe ſubjects, I will entertain thee with a ſtory.

There was a moſt ſurpriſing bird found in France, which *the learned* in that country dignified with the name of EMPYREAL SWAN. It attracted the attention of *the learned* throughout the world, but particularly the Engliſh WISE MEN. It had ſome particular feathers in its wings; its head was of a peculiar colour; and its whole plumage, indeed, was different from that of all other birds which Nature ever produced. There were ſuch *learned* diſſertations upon it, ſuch *grave* and *ſcientific* remarks — in ſhort, it attracted ſuch general attention, that the Royal Societies accounted it the greateſt wonder in the world; — nay, it was put at the head of the Seven Wonders of the World, making the EIGHTH Wonder! And, to ſay the truth, it was endowed with a *wonderful* faculty, which was, that whoever looked at it attentively, became infected with a French *mania.* — At laſt a perſon who had thought for himſelf, ſeriouſly ſcrutinized the opinions of others; who was *not* credulous, but who conſtantly oppoſed *French Wonders* with old *Engliſh Common Senſe* — this perſon, upon a careful examination of the Empyreal Swan, pronounced it to be a FRENCH GOOSE; which, when ſtripped of its borrowed plumage, it undoubtedly was. Upon this *the learned* ſet upon him with all the malice and virulence they were capable of; but he deſired them to ceaſe their *bawling*, for that they were all French *maniacs* together. More effectually to expoſe the deception, he began regularly to diſſect (or analize) this Bird of Birds; and, behold! he found it a CHEAT of CHEATS, which had made our *learned* the DUPE OF DUPES! In ſhort, it poſſeſſed the head, brains, voice, gait, and diſpoſitions of a Gooſe. —The detection of this impoſture was certainly a public benefit; for ſo entranced and infatuated were our philoſophers grown with this feathered idol, that they affectedly *aped* its habits; each of them had learnt to *cackle*, *gabble*, and TALK *like a GOOSE!* Yet, as the Roman Capitol was once ſaved by the cackling of Geeſe; ſo the Science of the world has been preſerved by *detecting* the cackling of a Gooſe!

But why ſhould I throw away wit + upon Geeſe, any more than *caſt pearls before Swine;* which I am forbid to do? This is, in my opinion, *The Age of Geeſe*—at leaſt it is ſo in the *field* of Science. And, worſe! I may juſtly add that it is the Age of *unprincipled* Geeſe. *O tempora, O philoſophiæ!* But *ſtart* not, *gentle* Reader! for if thy **HEAD** was not the head of a Gooſe, at leaſt thy **HEART** was the heart of a Hen,—otherwiſe thou couldſt not have ſuffered Science, Truth, Common Senſe, Reaſon, Honour, Integrity and Juſtice, to have been ſo cruelly trampled upon, after what I had told thee. Yet ſtill, recover thyſelf. Aſſert *now* their rights, or be for ever damned by thy children! Truth is now exhibited to thee, *diſtinct* as thy alphabet: therefore, with even the *brains* of a *Gooſe*, thou mayeſt open thine eyes and read. Thus,--I ſay G. ſtands for Gooſe.* *No*, ſay our *learned* Chemiſts, Reviewers and Journaliſts, *it ſtands for* WISDOM . . . . and hitherto thou haſt believed them, O credulous Reader! Many of our *Anglo*-French votariſts *know* that it ſtands for Gooſe, but want the integrity to acknowledge it, *reckoning upon thee being their dupe*, to keep up the cheat. Thou art therefore my only hope, to force them upon a public, candid, and fair diſcuſſion. *This is all I want.*

Mr. Davy alſo ſays, "The lighted taper, introduced into nitrous oxyde, burns at firſt as in oxygen; but afterwards with a flame, white in the centre, and blue at the circumference; a phenomenon apparently depending upon the nitrous acid formed in the proceſs."

Still this nitrous acid is formed!——But there is another proceſs where the carbonic acid is formed. He ſays, (page 286.), "Four cubic inches of this gas, when

+ The modesty of this poor deluded Zealot seems to keep pace with his Science in supposing ye above wit!

"perfectly decompoſed by the combuſtion of charcoal, "are converted into about 2.2 cubic inches of carbonic "acid, and 4.2 cubic inches of nitrogen,—no nitrous "acid being produced in the proceſs."

Does not the above clearly ſhew, that charcoal conſiſts, in part, of fixed air? From the ſteam of water being paſſed through it, it is plain that both fixed air and hydro-carbonic air are produced.

But, if it were neceſſary to ſay any thing more upon this ſubject, ſee an experiment of Dr. Prieſtley's, (given in page 38), and which the Dutch chemiſts repeated, and found JUST. In this experiment, by heating it, the gaſeous oxyde became all azote, and increaſed in volume, which will anſwer equally well if it be paſſed through red hot porcelain tubes. Now in this caſe the heat united the acid and the phlogiſton more intimately,—burning (or ſetting looſe) a part of it as free fire.

Again: Mr. Davy ſays, "Nitrous oxyde undergoes no "change in its compoſition at temperatures below thoſe "of ignition, though expoſed to them for ever ſo great "a length of time. When, however, it is paſſed through "a porcelain tube, heated red,—or when it is acted on "for a long while by the electric ſpark, a new arrangement of its principles takes place, and it becomes converted into nitrous acid, and a gas analagous to atmoſpheric air, but containing a little leſs oxygen *."

One of Dr. Prieſtley's experiments (as I have juſt obſerved) we have given in page 38, where the gas became

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* "Dr. Prieſtley firſt obſerved, that nitrous oxyde, after being acted "upon by the electric ſpark, became immiſcible with water; but he did "not notice the nitrous acid produced in the proceſs: neither did the "Dutch chemiſts, who repeated his experiments."

all nitrogen, and increaſed in volume. Mr. Davy's obtaining a different reſult, muſt have been owing to his expoſing the gaſeous oxyde to a greater heat, and by that means producing a partial combuſtion: for he ſays himſelf, that, in producing it from ammonia, when the heat is too intenſe, it will explode and form the nitrous acid. But, to ſet aſide all theſe puerile and abſurd reaſonings, expoſe this gaſeous oxyde of azote made from ammonia, to the nitric acid,—and it will readily imbibe it. And the acid, after imbibing or attracting it, will juſt be the ſame as if I had added as much in weight of pure nitrate of ammonia to the acid—that is, take a quantity of the nitric acid, and let it imbibe a certain quantity of the gaſeous oxyde of azote, and you will find they will moſt readily imbibe each other, they having a great attraction. And then take the direct ſame quantity of the nitric acid, and add to it the ſame weight of the nitrate of ammonia, to be exactly equal to the gaſeous oxyde.—And upon examining the nitric acids, after theſe impregnations, they will be found both the ſame—only a little more heat will eſcape from the one than the other, being that heat which aerialized the gaſeous oxyde.——*(See my Treatiſe on Air.)*—Need I ſay any more?

Mr. Davy likewiſe ſays, "Warm-blooded animals, when permanently immerſed in nitrous oxyde, live longer in it than in any other gaſſes, except ſuch as contain free oxygen; but cold-blooded animals are quickly deſtroyed by it."

And again—"The ſmaller quadrupeds die in pure nitrous oxyde in three or four minutes; they abſorb a little of the gas, and their death ſeems to be occaſioned by exceſſive ſtimulation."

Now, reader, it muſt needs appear a little extraordinary that an air formed of 37 parts of oxygen and 63 of azote, ſhould ſtimulate more than pure oxygen, as it is well known that animals live in pure oxygen, very well:—but canſt thou *ſwallow* that? And if thou canſt, *Gape wider ſtill*, for thou haſt more to ſwallow; although thy reaſon may receive its "death by exceſſive ſtimulation," *alias* SWALLOWING. No *ſtimulus*, even pouring the hotteſt oil into a man's ſtomach, (which is allowed by all our *old-faſhioned* phyſicians to poſſeſs more ſenſibility than the lungs), will kill in ſo ſhort a time.——And they alſo ſay, that where death takes place ſo immediately, by means of breathing noxious airs, *it is by ſuffocation*, the air wanting its vivifying principle. Fixed air kills not leſs ſuddenly, as the gaſeous oxyde of azote; and if the animal is examined, its lungs will ſhew the ſame indications or appearances. And an acid air cannot, I ſhould ſuppoſe, *Meſſieurs SOLOMONs*, kill from exceſſive ſtimulus——Exceſſive folly! Yet our philoſophers can breathe the nitrous oxyde! *Their* lungs then muſt be different from ſmall 'warm-blooded animals;' for it kills (they allow) *a ſmall calf* inſtantly; but the *great calves*, unannoyed, *cut capers*, and dance a French cotillon upon breathing it!——Heavens! what wonderful *lungs!* but what far more wonderful *heads!!*

MAN is a little ſtrutting animal; vain of his faculties, vain of all his *fancied* attainments,—but, chiefly, highly impreſſed with his Senſe and Reaſon. In this *infirm* condition, is it any wonder that the moſt vague flights of imagination, the moſt worthleſs *conceits*, ſhould arreſt his attention, and paſs current with him for Common Senſe and Truth? In this French theory we behold every thing that is wild and futile, and have to ſtruggle with a thouſand nameleſs abſurdities before we can reconcile ourſelves to any part of it. *'Tis a chaos of a wilderneſs.*— On your firſt entrance Common Senſe is ſhocked . . . . . '*Oxygen is the ſouring principle,*' turning phlogiſtic bodies, as ſulphur and phoſphorus, into the ſtrongeſt acids, though it be not itſelf *acid.* This alone is ſufficient to make a prudent man halt at the very threſhold. But then the body which poſſeſſes the greateſt quantity of oxygen, is water,—and water is not the leaſt acid, being the moſt ſimple and inſipid body in nature, although it poſſeſſes 85 parts in 100 of this air *(the ſouring conjurer)*, while the vitriolic acid only poſſeſſes 29 parts in the 100. The nitric acid has another baſe; viz. *azote;* very different from ſulphur or phoſphorus: yet it is an undoubted fact that one acid may be tranſmuted into the other. *Here is another mountain to remove.* But though the nitric acid is ſo ſtrong an acid, yet they ſay it is formed of the very ſame materials as our atmoſphere, and (Mr. Davy ſays) *in chemical union.* Sagacious Reader! doſt thou obſerve any analogy between them? Doſt thou require me to ſhew its abſurdity? They poſſeſs equal quantities of caloric, they affirm, although one is an aerial body and the other a ſolid. Why, there is, in my opinion, juſt as much difference between them as there is between an *old ſolid* philoſopher and a *new aerial* philoſopher. But as this conjurer *Oxygen* often plays us ſuch tricks, ſometimes being a *ſouring conjurer* and ſometimes a *ſweetening conjurer*, let us ſee whether *Azote* is a confederate conjurer, or not.—In ammonia he forms the alkaline principle. It muſt be *him*, and not *Hydrogen;* for *Hydrogen* is the baſis of water; and as it ſhews no alkaline principles, it muſt be this fellow *Azote:* it is for this reaſon that he is called *the alkaline principle,* and lately they have made him *the animalizing principle.* In ſhort, he is a more wonderful operator than *Oxygen;* ſomething more than co-equal in office; he is turned and twiſted to more wonderful feats. But ſtill he muſt imitate his ſuperiors; like them, he muſt have caprices and vagaries; for inſtead of becoming *the alkaline principle* always, he takes an eccentric turn, and becomes *the acid principle!* As in the nitrous acid.—— Therefore, reader, do not be ſurpriſed at our *great, wiſe, learned, candid,* and *honeſt* philoſophers, being bewildered and infatuated, when they had ſuch wonderful conjurers, ſuch nimble harlequins, ſuch dextrous, cunning and deceitful jugglers to follow;—no wonder they became tainted with the ſame *principles.* . . . ECCE HOMINES! *Learned philoſophers, conjurers, jugglers, harlequins,* &c. &c. &c.…………

Experimenters multiply. Indeed the old ones, as if conſcious they can gather no laurels in this field of ſcience, ſhew a diſpoſition to *ſkulk,* leaving it in the poſſeſſion of young *daſhing* experimenters. A Dr. Henderſon of Edinburgh has brought ſome experiments to prove the abſurd and improbable opinion that azote is abſorbed into the ſyſtem by reſpiration. His words are: "The opinion has been hitherto much controverted, but "which I flatter myſelf to have now ſufficiently aſcer"tained."—Pretty well in point of confidence! But as the French theory cannot err, and as Mr. Fourcroy ſuppoſes it the great animalizer, therefore it muſt be ſo.—But let us examine the gentleman's experiments—which I ſhall do very curſorily, for I am heartily tired of following and detecting every abſurdity.——He gives us three experiments, breathing 600 inches of atmoſpherical air, which was reduced to 570 inches. 17.7 cubic inches of azote were imbibed in one experiment, and twelve cubic inches in the other.—The next experiment was breathing 1000 cubic inches of air, and 15.1 cubic inches were abſorbed. So the average is about one-fortieth of the whole, or 1-32 of azote. *So much for accuracy.* And theſe experiments, he ſays, "were ſelected from many others." All pretenſions to *accuracy* in theſe experiments muſt be vain, by reaſon of the uncertainty of the exactneſs of the *inſpirations* and *expirations*, at the commencement and at the concluſion of ſuch experiments, which continued four minutes. His teſt for aſcertaining the quality of the air was *ſulphuret of lime;* and he is obliged to ſay, "The only objection to which the above teſt may ſeem liable, is, that (according to ſome) it abſorbs a ſmall proportion of the azote, as well as the oxygen of the atmoſphere." But, to vindicate his theory, he aſſerts not I ſhould ſuppoſe Dr. Henderſon not above one-fortieth of the whole. Such pretenſions to *nicety* in experiments of this nature, are truly laughable! They will be telling us ſome day of the WEIGHT of the MOON, even to *drams, ſcruples* and *grains*—nay, *to the very fraction of a grain!*—I wiſh there were infallible experiments to aſcertain the *quantum* of *brains* each man poſſeſſes, and every man's *integrity* and *candour:*—This is a *deſideratum* in ſcience which is moſt of all wanted; "a conſummation devoutly to be wiſhed:"—But, Mr. Reviewers, what would YOU weigh, in either ſcale? I ſuſpect that your *heads* would kick the beam; and if it ſhould even *break* them, there would be no great harm done. *Empty ſkulls!* the reſidence of no one quality that ſhould adorn or protect the head of a critic!

By all the trials that our *aerial* chemiſts could invent, they have found they cannot make a ſimilar air to atmoſpherical air, by mixing oxygen and azóte; ſhewing the ſame quality, either in combuſtion, reſpiration, vegetation, or with nitrous air. Bodies burning in this mixture of airs, (which they do with partial cracklings and exploſions), mice will not *live* in it with that vigour neceſſary to the ſeveral functions of *life*. Vegetables ſoon die in it.—All this I have found*.

The Monthly Review, ſpeaking of Mr. Humboldt's publication, has the following obſervations:—"The mixture of the nitrous with the oxygenous gas, affords not ſuch regular reſults as that with atmoſpheric air;—nor does the artificial compound of 27 parts of oxygenous gas, and 73 of the azotic,—manifeſt on trial the ſame properties as the air which we breathe. Theſe facts betray the lameneſs of received principles, and excite ſuſpicions with reſpect to the legitamacy of ſome capital analyſes †."

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* Mr. Humboldt found, that the atmoſphere upon high mountains was not ſo pure as in the plains; and Mr. Saſſure found a great quantity of fixed air in the atmoſphere, *on Mount Blanc*, lime-water becoming immediately turbid by it.

Guyton has given a deſcription of an ore of antimony, lately diſcovered in the province of Gallicia, being a ſulphuret oxyde. They might find many of ſuch bodies *oxydes*, without oxygen.

† I ſhall quote an accurate experiment of Doctor Prieſtley's, (vol v. page 161), who ſays:—"To make the experiment in the moſt unexceptionable manner that I could contrive, I, in the next place, got two mice, of nearly equal ſize, and put them into exactly equal quantities, viz. about five ounce meaſures, of the ſame deplogiſticated air, (the meaſure of its purity, with two equal quantities of nitrous air, being 0.42), in nearly equal and ſimilar glaſs jars—one ſtanding in lime-water, and

Now let us accurately examine theſe experiments of Dr. Prieſtley.—The mice laboured for ſome time with difficulty of reſpiration, though the air was ſo pure when they were taken out of it as 0.96 and 0.98—conſiderably purer than the atmoſphere. And another eſſential fact

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the other in common water. Both the mice continued in this ſituation ſomething more than two hours and an half, after which the air, which had been confined by lime-water, appeared to be reduced in the proportion of 9 to 5¼, the meaſures of the teſt being 0.96; and the air which had not been confined by lime water, was diminiſhed in the proportion of 9 to 6½, the meaſures of the teſt being 0.98. Both the mice, though kept pretty warm, laboured alike with a difficulty of reſpiration, ſome time before I put an end to experiment. In the courſe of it I agitated the lime-water a little now and then, in order to make it abſorb the fixed air the better, by admitting freſh lime-water to the air that had been reſpired.

"It appears from this experiment, that the air confined by lime-water was both diminiſhed and phlogiſticated, exactly like that which had been confined by common water, by the reſpiration of mice of equal ſize, in the ſame time. The diminution indeed was, at firſt, a ſmall matter greater in the air confined by the lime-water; becauſe the common water did not imbibe the fixed air ſo readily; but this made no apparent difference with reſpect to the mice; and the next day the two portions of air were found to be as nearly as poſſible of the ſame dimenſions, and of the ſame degree of purity.

"In the preceding experiments, and ſeveral others which I made about the ſame time, I found that mice would not live in dephlogiſticated air till they had completely phlogiſticated it, though they lived longer in it than, in proportion to its purity, with reſpect to common air; and for this I cannot aſſign any ſufficient reaſon. I had once imagined that this was owing to my being obliged to make the mice paſs through a quantity of water, by which the air was confined; but I put a mouſe through the ſame water into a quantity of common air, and it lived in it till it was thoroughly phlogiſticated. This may deſerve a farther inveſtigation I ſhould have put other mice into what remained of the dephlogiſticated air."

he informs us, that " they lived longer in it than, in proportion to its purity, with reſpect to common air; and for this (he adds) I cannot aſſign any ſufficient reaſon." Now our *aerial* philoſophers, as they do not heſitate to account for every thing, aſſert with the utmoſt *non chalans* that the reaſon is, that the oxygen kills from its ſuperior *ſtimulus* — that the gaſeous oxyde of azote made them dance to the tune of *niddy-noddy*, from this ſame cauſe; though they ſay that it only poſſeſſes about one-third of oxygen to two of azote, and " kills (as Mr. Davy ſays) mice in three or four minutes." Nevertheleſs, to make the wonder (*alias* abſurdity) ſtill the greater, the oxygen and azote are chemically united, and the poor mice have to break this chemical union; who, notwithſtanding, do break it, and with ſo much vengeance as to receive ſo much of the *vital* principle, which then becomes the *deadly* principle. And yet, to make our *ſervum pecus* STARE a little more, the mechanical application of the gaſeous oxyde to the blood of the mice, muſt be leſs as it is a lighter air than oxygen gas. But, *this is a trifle!*— And now, *ſervum pecus*, though each of you were as credulous as an *aſs*, and had the throat of a *whale*, to gulp all,—yet you muſt not ſwallow all; for if it appears from Dr. Prieſtley's experiments, that the oxygen gas was received in leſs proportion into the blood of the mice than when they breathe the common atmoſphere, (for it ſerved them longer in proportion to its purity), conviction *muſt* be the conſequence. Our moſt *learned* French and Engliſh philoſophers, in that caſe, can no longer throw out a tub to amuſe you, or blind you: for though your brains were as impervious as a mill-ſtone, you muſt now be ſenſible that the pure part of the atmoſphere and the oxygen of the elaboratory are not the ſame body*. Alſo in firing hydrogen gas in oxygen gas, it is well known that fixed air is formed in the experiment, the heat being ſo intenſe as to decompound the oxygen gas's acid from its aerial ſtate; but when hydrogen gas is fired in the atmoſphere, I have found fixed air in the reſiduum, even though it was previouſly paſſed through lime-water.

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[*] This conjurer, Oxygen Gas, though we cannot force him through membranes, or the coats of a bladder, (a well-known fact); yet when in the lungs, and perfectly concealed from every human eye, he then daſhes through the membranes, and fixed air does the ſame; only as one enters, the other makes his exit, playing at *Bo-peep*. O what *Hocus-pocus* work! Theſe are far more extraordinary feats than Harlequin jumping through a barrel:—Tuſh! the Stage Harlequins are nothing to our philoſopher's Harlequins; they are ſuperior even to the Bottle Conjurer—though, to ſay the truth, there is reaſon to ſuſpect that our philoſophers have ſtolen ſome of their ideas from him; for Mr. Morveau propoſes to *bottle* oxygen, and, in order to this, he firſt coaxes him to enter the marine acid in the ſhape of nitrous vapour, (Dr. Prieſtley having ſhewn that it makes the ſtrongeſt oxmuriatic acid): then, opening the bottle, the rogue jumps out and, leaving the marine acid, takes the perſon who holds the bottle by the noſe, which he enters, and jumps down his throat! Daſhes through the membranes of the lungs, raviſhes his blood, giving him the moſt delightful and ecſtatic ſenſations, ſuch as were received at the Inſtitute,—dancing and capering to the well-known tune of FOOLS ALL, FOOLS ALL. Now, as I have already ſuggeſted the burning of the enemy's gun-boats, I tremble at the idea of his coming over in boats conſtructed to ſail under water, navigated by ſoldiers and ſailors, each of them furniſhed with a bottle of the oxmuriatic acid—which eclipſes the American plan. The only hope, in that caſe, for our our ſalvation, is, that it will make them dance and caper ſo, that they will be utterly exhauſted before they reach this ſide of the water.

This moſt extraordinary quality which the conjurer poſſeſſes, of making our *ſolid*, *grave*, and *profound* philoſophers dance ſo, would have alſo the ſame effect upon every animal; we ſhould ſee even the leaſt volatile of

I flatter myſelf that the reader is now fully ſenſible of the reaſonableneſs, juſtneſs, propriety and beauty of my doctrine,—the diſtinguiſhing feature of which is, that it accounts *rationally* for all the phenomena. Let us then examine a few experiments, and, I truſt, I ſhall forcibly impreſs him that all the acids may be formed into each other.—— By burning ſulphur in the gaſeous oxyde of azote, they form together the vitriolic acid. By burning phoſphorus in this gas, they form together the phoſphoric acid: nitrous gas does the ſame. In the nitric acid, applied to ſulphur and phoſphorus, nitrous air is formed

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the brute creation [an *aſs*, a *gooſe*, &c.] as apt at a *jig* as a dancing-maſter, were it not that, to counteract all this merriment, Nature has formed two other conjurers, [HEAVY DOGS!] to ſtop their capering; for as ſoon as this Merry Andrew, *Oxygen*, enters the blood, ready to receive him are two ſolid conjurers, *Carbon* and *Hydrogen*, who ſeize the merry tumbler, chain him down, and make him as quiet as a mouſe! And would you believe it, gentle reader! that this ſkipping conjurer, this ſouring conjurer, is wrapped up in the oxmuriatic, nitric and vitriolic acids—— which, of all the bodies in nature, contain him in the greateſt abundance. And the body that holds the two ſleeping conjurers, or what keeps the ſkipping one quiet, is nothing leſs [*bear it, O ye ſages!*] than BRANDY:— for ſpirits are formed of no other bodies than carbon and hydrogen.—— Who would have ſuppoſed this?—Go, ye fools! you are no philoſophers. Doubtleſs *your* heads and heels would have been ſet a capering with ſpirits, and your ſtomach ſet a griping with acids. But *ye* are common mortals, with common heads, heels and ſtomachs,—very different from our wiſe immortalized philoſophers!

There is the ſame wonder alſo in animal putrefaction:—the body becomes highly alkaline, while the air becomes acid. Then, according to their doctrine, the principle of alkaleſcency makes the air become acid; and the ſolid body, by parting with this principle, becomes alkaleſcent. Heavens! what phenomena! Is it poſſible to reconcile theſe contradictions (*alias* abſurdities)? I call for an explanation, and certainly the philoſophic world ought to demand one. Hitherto I have called in vain, by a part of the acid; the other part entering the ſulphur and phoſphorus, forming both the vitriolic and phoſphoric acids. That when ſulphur and phoſphorus are burnt in the atmoſphere, the aerial acid of the atmoſphere, (that is, the fixed air) is condenſed, and forms, with the ſulphur and phoſphorus, both the vitriolic and phoſphoric acids. That when the oxmuriatic gas burns phoſphorus or ſulphur, it does the ſame, forming them into the phoſphoric and ſulphuric acids. Then, clearly, the nitric acid aerialized as the gaſeous oxyde of azote or nitrous gas, the carbonic acid and the marine acid, all of which form with the ſulphur and phoſphorus, the vitriolic and phoſphoric acids. This is perfectly rational and conſiſtent, and what we might have expected *à priori*. Dr. Prieſtley (vol. vi. p. 272.) turned a great quantity of the vitriolic acid air into fixed air, which lime-water abſorbed—and the reſiduum was ſometimes weak inflammable air, and at other times ſo ſlightly phlogiſticated as to form a kind of azote.

This decompoſition of water, founded upon the miſtaken experiments of the French in firing hydrogen and oxygen, enters into every explanation of chemiſtry; thus it is made the expounder of all chemical phenomena—in a word, it is the great elucidator of all myſteries, the baſis of French chemiſtry!——Let us examine ſome experiments of Mr. Accum, and alſo of Dr. Briggs, upon the combuſtion of ſulphur and phoſphorus.—Sulphur and phoſphorus are two high phlogiſtic bodies, which will unite together; but then it requires the greateſt caution, at a temperature not exceeding 180, and that by the aſſiſtance of water. Upon their uniting, white fumes ariſe,—which, as they approach the air of the atmoſphere, they burn—*but not without they approach the atmoſphere*, as Dr. Briggs allows. For he ſays, "The firing only happens after the ſulphur phoſphorated hydrogen reaches the external gas;"—adducing this as an argument, that the water is not decompounded according to the way which Mr. Accum explains it. For he ſuppoſes that the water is decompounded, and that the hydrogen and oxygen eſcape. But if that were the caſe, (as Dr. Briggs obſerves) "If the oxygen was alſo allowed to eſcape, we ſhould feel its preſence, from *exploſion upon exploſion* taking place, immediately after its leaving the water, in company with the hydrogen, united with as much phoſphorus as is ſufficient to ſet fire to the two." But no combuſtion takes place till the fumes have acceſs to the oxygen of the atmoſphere. But, ſtill true to the French theory, Dr. Briggs ſays the water is decompounded, and its oxygen is attracted by the phoſphorus and ſulphur. According to this opinion, then, we ſhould by theſe means form the vitriolic and phoſphoric acids. But they are ſtill ſulphur and phoſphorus.—By continuing this compound in heated water, we ſhould certainly be able to form theſe acids, as the ſulphur and phoſphorus may (in a manner) be all aerialized with the hot water, and ſtill no oxydes *in* the water.

But there is another huge and enormous chaſm for our French theoriſts to get over. If the drieſt ſulphur and phoſphorus are expoſed to a ſtrong heat, in cloſe veſſels; either with pure azote, fixed air, or hydrogen; or even in cloſe veſſels, WITHOUT AIR,—where the credulous French maniacs cannot ſay there is the leaſt particle of oxygen; and, expoſed to a ſtrong heat, they explode.... The only way they can explain this, is, that this conjurer

Oxygen, can penetrate through the ſides of the ſtrongeſt veſſels. In ſhort, he is *a conjurer;* therefore, to argue about him as an ordinary agent, is not fair. Dr. Briggs inſinuates that there may be ſome particles of the atmoſphere which may remain between theſe particles of ſulphur and phoſphorus. But if they are each in a ſolid lump, pray, Doctor, can you ſuppoſe there to be any atmoſpheric air in them? You may as well ſuppoſe the Moon is a Cheſhire Cheeſe. Is it befitting the dignity of a rational philoſopher, to argue againſt ſuch ſuppoſitions?—But, ſolid Reader! (for if thou belongeſt to the *aerial* tribe, I ſhall have nothing to do with thee)—theſe two ſtrong phlogiſtic bodies, by being united, at the moment of their union produce a great degree of heat. I demonſtrated this in my letter to Mr. Cavendiſh, when, upon ſulphur and iron or copper uniting, at the moment of their union they heated the bottom of the crucible red hot. My explanation is this:—Theſe two bodies are formed of a great quantity of fire, as the vitriolic acid and the earth of the metals are united to a great body of fixed fire. When they are thus united together, a great quantity of their fixed fire is ſet looſe; which is in ſo conſiderable a portion as to ſet fire to them, making an immenſe exploſion. But all this is effected *without the aid of the conjurer* OXYGEN; there is not the ſmalleſt pretence of his being preſent: ſo that, in this inſtance, they are hard put to it to make a whale for the vulgar herd to ſwallow.—When they unite in heated water, as the fire is generated by their union, it aerializes them, receiving from the water the water of compoſition,—ſo that they form what they call their *phoſphorated and ſulphurated hydrogen.* But if they unite too haſtily, the heat will be ſo conſiderable as to make them explode; therefore it requires great circumſpection in the experimenter. Now, individuals of this formidable combination! ſingly and collectively, I challenge you upon theſe experiments; I pledge myſelf to demonſtrate the groſs abſurdity of the compoſition of water, upon them alone. *I dare you to the combat.*——Reader! what an irkſome taſk have I undertaken, to force open thine eyes to theſe abſurdities! (which drudgery I have patiently ſuſtained for upwards of twenty years.) But there is an old adage which is perfectly applicable to the buſineſs in hand:—“*There are none ſo blind as thoſe who will not ſee*[*].”

Upon ſulphur and the metals uniting together, they each give out a great quantity of their fire *immediately upon their union*, turning the bottom of the crucible *inſtantaneouſly* (in a manner) *red hot:* and, as ſulphur is the vitriolic acid united to a high ſaturation of fixed fire, and the metals a metallic earth united to a great quantity of fixed fire, *we might* à priori *ſuppoſe this.* Now, when the ſulphur and the metal unite, the vitriolic acid and the earth of the metal require a leſs ſaturation of fire when united than when ſeparate; in conſequence, a great part of their fire is ſet looſe: as when ſulphur and mercury are united, (ſee page 97), they produce an active flame, and form cinnabar. Now, that the mercury has loſt its metallic fixed fire in cinnabar, is certain; as its operation upon the human ſyſtem is ſo very active, and every one knows that mercury, in its metallic ſtate, is *perfectly inactive.* If there be any *heads* that diſpute it, I would have them to be ſalivated with cinnabar;—and I am not without hopes that the mercury would drive out of them theſe high-flown vagaries. Indeed I I think their heads *demand* ſuch an operation, to ſuperinduce the faculty of *right reaſon*, and to *prepare* the mind for the reception of every thing that is rational, candid and juſt.

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[*] I have often wondered how this monſtrous French theory could overturn every thing that our forefathers have taught us. And even Mr. Lavioſier's great experiment, which was accounted to be ſo decicive, is, upon inveſtigation, truly ridiculous. Upon adding the nitrous acid to mercury, nitrous air is generated. Now, they ſay that in the proceſs the nitrous acid is decompounded, its oxygen going to form the calx; but Mr. Metheric ſays, (page 146), “Though nitrous air is obtained from “a ſolution of mercury in nitrous acid, I found *almoſt all the acid* in the “ſolution.” And Dr. Fordyce aſſerts that he “found *almoſt all the acid* in “the ſolution of zinc in the vitriolic acid.”

This happy iſland, celebrated for its wiſdom and liberality, in the ſciences as well as the arts—what a figure will it now cut in the eyes of ingenuous foreigners!═══ The hiſtory of literature, I am confident, does not furniſh ſuch an example, even in the moſt barbarous ages, of diſingenuouſneſs, inſult and injuſtice, as I have experienced. To what a dilemma are our Engliſh philoſophers driven! "*How are the mighty fallen!*" If they retain the leaſt particle of honour or principle, they will retract; or (which is the ſame thing) they will be forced upon a fair, candid, and public examination of my ſyſtems—*or be damned.*

It is wonderful how tenacious they have been of that abſurd doctrine of *the compoſition of water*—whereas, if they had once but reflected what an extravagant idea they were entertaining, and how every phenomenon in Nature contradicted it, they readily would have abandoned it. But, no! Their conduct in this inſtance was of a piece with all the extravagant parts of their ſyſtem, which are *Laputatic* and *lunatic*, exceeding even the imaginary flights of the Arabian Tales. In many proceſſes of chemiſtry the ſmell of the nitrous acid appears, and alſo of the vitriolic acid, and likewiſe the oxmuriatic acid, and ſometimes of the volatile alkali—in all which caſes it is univerſally allowed that theſe bodies are preſent. Now, the hydrogen gas, from the ſolution of metals in the vitriolic acid and water, has always a ſtrong hepatic ſmell, as Mr. Paul obſerves: (ſee page 74.)—— It was this identical hepatic hydrogen gas, of which the French made their boaſted experiments, firing it in oxygen gas: there was no acid, it ſeems; but water was in the reſiduum. Now the hydrogen gas poſſeſſed the vitriolic acid; then what became of it? Undoubtedly it eſcaped with the fire in the combuſtion! Then if we are certain that an acid can eſcape in the combuſtion, need we be ſurpriſed at the acid of oxygen gas eſcaping? For here the fact is clearly aſcertained, of the vitriolic acid eſcaping which was in the hydrogen gas; as it was not, *after* the combuſtion in the water? Can any reaſoning be more forcible than this, ye *Laputatic* Chemiſts!--- Dr. Fordyce, by the moſt accurate experiments in diſſolving zinc in the vitriolic acid and water, found, in forming nine grains of inflammable air, a loſs of 1½ grain of the vitriolic acid, which muſt, according to his experiments, have gone to the formation of the inflammable air.—*See the Philoſophical Tranſactions for* 1792, page 382.

As the gaſeous oxyde of azote acts in combuſtion the ſame as oxygen gas, it follows that an elucidation of it muſt facilitate the elucidation of the other*.

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* Dr. Higgins ſays, page 164, " Dr. Prieſtley has diſcovered a ſpecies of nitrous air which ſupports combuſtion, deſtroys animal life, and is con-

Now it appears that this air is obtained ſrom iron, without any water. But we need not be ſurpriſed that the calx of iron will attract its phlogiſtic part, turning

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denſible in water. This he has called dephlogiſticated nitrous air. I conſider dephlogiſticated nitrous air to be the laſt ſtage of nitrous acid, and to be leſs underſtood than the four preceding. I expoſed four equal quantities of nitrous air in different tubes, to a nearly equal proportion of iron and water. In three weeks the air was diminiſhed one-ſixth, and the reſiduum extinguiſhed a candle, and reduced common air. In three weeks more it was reduced about one-fourth, and the reſiduum ſuffered a candle to burn in it faintly. When it ſtood a fortnight longer, the diminution was nearly one-third of its bulk, and a candle burned in the reſiduum with an enlarged flame. I let the other tube ſtand until the air contracted to more than one-third of its original bulk; the reſiduum was phlogiſticated air, and had the ſmell of volatile alkali. From the progreſs of theſe experiments I did not heſitate to conclude, but that which is called dephlogiſticated nitrous air, is common nitrous air, deprived only of a portion of its dephlogiſticated air. Dr. Prieſtley found that nitrous air, which ſtood in contact with iron and water for four months, extinguiſhed a candle. He likewiſe found that a candle burned with an enlarged flame in nitrous air which had been in contact with iron, over mercury, about ſix months. The ſame philoſopher found, that nitrous air, expoſed to liver of ſulphur for a day, was diminiſhed one-third of its bulk; a candle burned in the remainder with an enlarged flame, and it was not in the leaſt diminiſhed by nitrous air. I have frequently obſerved that nitrous air, when reduced to two-thirds of its bulk, always admitted a candle to burn in it with an enlarged flame; but that in proportion as it got below this ſtandard, it ſupported flame ſo much the worſe, until it was reduced nearly to one-third, when it extinguiſhed a candle."

Dr. Prieſtley alſo ſays, in his 6th volume, page 317, "On the 26th of May, 1782, I examined a quantity of nitrous air, which had been confined by mercury with iron ſhavings from the 27th of Auguſt preceding, when I found one-half of it abſorbed, and a candle burned in the remainder better than in common air, though a mouſe died in it; and yet this air had remained ſeveral months in the ſame ſtate, with reſpect to quantity, in which it was when I examined it; ſo that I concluded it would never it into azote. By waſhing it in water it will do the ſame as Dr. Prieſtley has deſcribed*.

Now here the diſtilled water robbed it of its phlogiſtic part; and if it be expelled again, it is ſtill the ſame air, only more pure — as "a candle burnt in it with a vivid flame, as before." And alſo the air, left unabſorbed by the water, was what they call azote — as "a candle went out in the remainder." But where this air was not expelled from the water, Berthollet "found it was nitrate of ammonia." *Can any thing be more fully and clearly elucidated?*—What the calx of iron attracts from this air is alſo the nitrate of ammonia; and the remaining air is the ſame as is left upon agitating water in it, I found.

Phoſphorus burns in atmoſpherical air, in the temperature of the atmoſphere; but it will not do the ſame in oxygen gas, nor in azote and oxygen gas, mixed in any proportion. Alſo Homberg's pyrophorus would burn in nitrous air, which would not burn in the gaſeous oxyde of azote, as Dr. Prieſtley found: *ſee his 6th volume,*

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have extinguiſhed a candle, in conſequence of any longer continuance in that ſtate. In this caſe, however, there was no ſenſible quantity of water at all; whereas in the other there were a few drops."

See alſo the 5th volume, page 111, where a very minute account is given of the proceſs.

* He ſays, (vol. v. page 214), "On the 13th of October I repeated this experiment, by agitating a quantity of this nitrous air in a ſtate in which it did not affect common air at all, in diſtilled rain water; and, expelling the air *immediately afterwards*, a candle burnt in it with a vivid flame, as before; and after the water had abſorbed what it would of the air, a candle went out in the remainder, ſo that it was, in all reſpects, the ſame thing when it came out of the water, that it had been before it went into it."

page 335.—Here the acid of the nitrous air is neceſſary to expel the fire of the pyrophorus, which then ſets fire to the whole. If you drop a drop of the nitrous acid upon pyrophorus, it will turn it red hot as a coal, as Mr. Bewley found for the ſame reaſon. It is from this that nitrous air burns phoſphorus ſpontaneouſly, when the gaſeous oxyde of azote will not *.

What canſt thou ſuppoſe, Reader, when they are obliged to ſet aſide the moſt obvious and clear explanations? Thou mayeſt eaſily put it to the teſt of *experiment.* Take a few drops of the fineſt oil, and, with the aid of a little cotton wick, burn it in oxygen gas, and the gas will be turned into fixed air. Burn the ſame quantity of oil in the nitrous oxyde of azote, and the gas will be turned into the nitrous acid, nitrous air, and azote. In both caſes they will burn with ſimilar brilliancy, very different when the oil burns in atmoſpherical air.—Now, my good Reader, what has become of the oil's carbon? The oil is equally conſumed in both combuſtions. Indeed, as Dr. Prieſtley ſays, " they burn like each other."

I ſee from the Reviews, that Dr. Black and his Editor are angry at our *aerial* chemiſts, in attempting to carry this wild French theory into the different phenomena of nature, ſeeing that I have ſhewn their attempts to be abſurd and abortive. But I had ſhewn it equally as abſurd in accounting for the phenomena in the elabaratory. At leaſt, *now* I think I have done it ſo completely, that the *ſervum pecus* muſt open their eyes; the whale will be no longer ſwallowed. CRAM will be no longer the order of the day; but REASON, COMMON SENSE and JUSTICE, will, I hope, aſſume its place.

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* Dr. Prieſtley ſays, vol. ii. pp 130, 131: " In order to judge how far an acid prevailed in this air from ſpirit of nitre and oil of turpentine, I put alkaline air to it; when inſtantly a white cloud was produced, which roſe to the top of the veſſel ——Repeating this experiment ſome time afterwards, about one-fourth of the mixture of this air and alkaline air, diſappeared upon their being put together. Half of the remainder was abſorbed by water; and in this ſecond remainder, which, by its redneſs, on being expoſed to common air, appeared to be conſiderably nitrous: a candle burned with a beautifully enlarged flame."

The Critical Review alſo ſays, "The chemical natures of phoſphorus and ardent ſpirits are admirably explained; and under the latter head we find a full account of æthers, which, in Dr. Black's opinion, differ from alcohol only in containing a proportion of the acid uncharged with the alcohol." Now I think they are neutralized, which I have urged as an argument againſt the French theory; and let me aſk Mr. Davy if the nitrate of ammonia is decompounded by barely volatilizing it? It being formed of the two moſt volatile bodies in chemiſtry, ſhould not the æthers alſo be decompounded? No. If there exiſts a fact in chemiſtry, they are the acid and the volatile alkali, in one caſe; and the acid and the alcohol, in the other, juſt volatilized into the ſtate of gas. There is no more decompoſition of their ſuppoſed elements, than there is of water being evaporated. Dr. Black ſays that oil becomes rancid, he thinks by abſorbing oxygen. This *ſouring conjurer*, inſtead of acting the part of a ſouring acid, here then muſt act as a heating alkali. But will not acids, *ye conjurers!* take off this rancidity of oils?—Mr. Henry found that fixed air would take off their rancidity. The oxygen gas acts by communicating to the oil its fixed fire, by that means becoming itſelf fixed air, and the oil becoming both hot and nauſeous, from receiving this additional fire. Dr. Black 'ſuſpects that manganeſe attracts azote from the air; and this is rendered probable (they ſay) by an obſervation of Sequin, who found manganeſe to yield azotic gas in low heats.' Does not this inconteſtibly prove to thee, Reader, that in theſe '*low heats*' its fixed air only gets a ſaturation of fire, ſo as to form azote, and not oxygen. But if thou wilt take pieces of the ſame manganeſe, by giving each piece different heats, thou wilt receive fixed air, azote, and oxygen gaſſes, —in ſuch degree as thou pleaſeſt to proportion thy heats. According to Dr. Black, white-lead is not a pure oxyde, but in ſome meaſure a carbonate. This, I think, I have fully and clearly elucidated. But take, Reader, this "white carbonated lead," and roaſt it for 48 hours in cloſe veſſels, and thou wilt turn all this fixed air into oxygen gas and azote.——All theſe quotations are taken from the Critical Review for June, 1804, which concludes with a *bright* ſimily, as they ſuppoſe; but I think it a very *dark* and *black* one.——They ſay, "The new chemical ſyſtem of the French philoſophers reſembles the *Genius* of the Arabian Nights Entertainments: when introduced he is a vaſt cloud of ſmoke, which obſcures every object." He has done that with a vengeance! He has *obſcured* and taken away the wits of our poor philoſophers, and left nothing in their place but *blackneſs* and ten-fold *darkneſs*. I flatter myſelf, however, that I have *routed* "the *Genius*," that I have diſſipated the "*ſmoke*," and made all as clear and as bright as the noon-day!

Indeed, ſome years ago, I compared the French theory to an Arabian tale; for it is an *aerial* fabric, and is *rich* in all the wild extravagancies that diſtinguiſh an eaſtern ſtory*. Like an Arabian tale, too, it is the *ſublimity* of *nonſenſe!*

The volatile ſalts in the bile become cauſtic by being expoſed to the atmoſphere. Here, then, according to them, the bile acquires its cauſticity from imbibing oxygen. If the volatile ſalts be prepared with quick-lime, they attract the fire of quick-lime, and likewiſe become cauſtic. How then could they get oxygen from quick-lime? My theory ſays, *Both theſe phenomena are produced from fixed fire, which the bile got from the air, and the volatile ſalts got from the lime.* Can any thing be more rational and beautiful!

From the circumſtance of Lavoiſier taking up the doctrine of *latent heat*, of which Dr. Black ſuppoſed himſelf the father, and who is ſo extolled as a *profound* chemiſt, became, in conſequence, a warm partizan of the French doctrine. When ſpeaking of the metals giving out violent heat, he ſays it is owing to their latent heat eſcaping. He might as well have ſaid, *latent folly.*—— Doctor Crawford, who, along with Dr. Irving, were the fathers of the doctrine of *different bodies* having *different capacities for heat*, ſhewed that the calx of a metal had a conſiderably greater capacity for heat than in its metallic ſtate. Now, if Dr. Black, who acknowledged this capacity, had but aſked himſelf the queſtion, he muſt certainly have pronounced, that *the metals had not acquired greater metallic properties by giving out this heat.* But, in ſhort, this *latent heat* has been a *latent* cloſet, to conceal *latent* ignorance. *Know this*, Reader, (and perform the experiment thyſelf, without truſting to any prejudiced French votariſts), that *iron, when violently hammered, will become red hot;* and that, *after the operation, this iron will be found leſs ductile**; and beſides, *it will, with the vitriolic acid and water, give out leſs hydrogen.* The ingenuous Reader will be ſatisfied, that this muſt be owing to its loſing, by its hammering, a part of its phlogiſton. The adverſaries of this hypotheſis pretend that the reſult is to be attributed to *a partial calcination*, the iron imbibing, during the hammering, oxygen gas. But I have ſhewn the contrary in my *Chemical Eſſays*, to which I refer the

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* I cannot refrain from giving the EXULTATION of the Reviewers. —"The ſubject of balloons (ſay they) is alſo introduced; and we find, with peculiar ſatisfaction, that the reaſon which we alledged very early againſt their probable utility, are the ſame which Doctor Black, with his uſual preciſion, has urged. We muſt be allowed, with conſcious pride, to aſſume this credit; while every witling, every inſignificant ſcribbler, ſeems anxious to 'ſhoot his bolt' at a Reviewer."——Indeed it is hard that thoſe *learned, profound,* and *impartial* critics, ſhould be thus uſed.—Whether Dr. Black or the Reviewers had firſt the honour of theſe *wiſe* arguments, Time muſt develope. But let them have the honour of it in their own element, (AERIAL). With every man of *ſolid* ſenſe, however, that I have converſed with, the ſuppoſition of *utility* was ridiculous in the extreme! But, in ſhort, this is a *balloon age*. . . . You ſee, Mr. Critics, that I do not omit mentioning any thing for your honour: but your treatment of me has been very different. The fame of your wiſdom, in this *balloon caſe*, ſhall be tranſmitted to future ages,—to terrify "every witling" who dare oppoſe himſelf to your authority,—yea, every "inſignificant ſcribbler" who ſhall preſume to "ſhoot his bolt" at your empyreal chair—a chair encircled with Wiſdom, Candour, Honour, Integrity and Juſtice, enough to frighten the *Diable* himſelf!

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* Is it not well known to every common artiſt, that iron, by being violently hammered, becomes leſs ductile; and, in order to reſtore its ductility, it muſt be heated in the fire again. Alſo it is equally well known, that the more a metal is metallized—that is, the further removed from a calx,—the more ductile it is. But, in ſhort, this French theory ſtrikes againſt all the moſt obvious and the moſt firmly-eſtabliſhed facts, making *White* BLACK.

Reader.—Now ‘it is owing to the latent heat eſcaping.’ I am certain of one thing····If theſe philoſophers poſſeſs any ſenſe or reaſon, the French theory has made them become *latent; (id eſt)*, theſe excellent qualities are no where to be *ſeen*, *felt*, or *underſtood*. Pray, Meſſieurs *Crammers* and *Crammed*, (the *Deceivers* and the *Deceived)* if, even in cloſe veſſels, I mix mercury with melted ſulphur, they will produce inflammation: and as Mr. Prouſt ſays, (ſee page 97), “ The mercury is *not* oxygenated in cinnabar.” Therefore to ſhew that it has loſt ſomething more than its latent heat, I would have our philoſophers who can ſuffer ſo much *cramming*, to be *crammed* with cinnabar; in which caſe I am not without hopes that the ſalivation produced would diſcharge all theſe crudities with which their poor weak heads have been for theſe twenty years oppreſſed. They all require a thorough ſalivation or cleanſing; for it is not all ignorance, but in part it is the *leaven* of an illiberal temper, malevolence, jealouſy and injuſtice.

Whether indeed Mr. De Luc or Dr. Black is the father of this doctrine of *latent heat*, I preſume not to judge, being ignorant of the facts upon which they reſpectively found their priority; but I lament that Dr. Black ſhould be ſo puffed up with a *diſcovery* which is no better than an *ignis fatuus*, a *Will wi'the Wiſp!*

I think I have ſufficiently proved, that when the gaſeous oxyde of azote is formed, there is no decompoſition of water in the proceſs—that abſurd doctrine which they have recourſe to on every occaſion, when they undertake to explain the phenomena. Now, can the moſt bigotted and credulous ſuppoſe,—that when the nitrous air is formed into the gaſeous oxyde of azote, from ſulphur and iron, and alſo liver of ſulphur, that water is decompounded in the proceſs? But, *(to nail them cloſely, to force, them to reaſon)* Mr. Kirwan turned nitrous air into the gaſeous oxyde of azote, by mixing it with hepatic air—*that is*, their ſulphurated hydrogen gas. Now, in this gas there is no water: they ſay, *therefore none to decompound; and its formation then could not be from the decompoſition of water.* It is truly too ridiculous a doctrine to waſte ſo much time upon! For, inſtead of the nitrous air *receiving* oxygen, it muſt, according to them, have *parted* with oxygen, to unite with the hydrogen, which kept the ſulphur aerialized, as the ſulphur was precipitated he found: Therefore it muſt have united with the oxygen of the nitrous air, and formed with it water, according to the French creed, the immortal theory!—Hence it follows, that the needles in Dr. Prieſtley's experiments, when they formed nitrous air into the gaſeous oxyde of azote, muſt have furniſhed the hydrogen to unite with the nitrous air neceſſary to form it, *alias* the phlogiſton, turning the acid air from ſour to ſweet, *that is*, neutralizing the acid. And this forcibly teacheth, that bodies becoming ſweet is by phlogiſton, or fixed fire forming the ſaccharine principle. *Now all this is written in as plain characters as* A, B, C.; *ſo plain, that* 'he that runs may read.'

In *Nicholſon's Journal for Auguſt*, 1804, Dr. Thomſon has given a long diſſertation upon the oxydes of lead, which I ſhall examine.—He begins by noticing the yellow oxyde, *(maſſecot)*, concerning which he ſays, "But the eaſieſt method of forming it, is to diſſolve lead in nitric acid." Notwithſtanding the preſent aſſertion, this *maſſecot* yields air, one-half of which is fixed air, according to Doctor Prieſtley's experiments: (ſee page 11.)—Whether the calx of lead contains the nitric acid or the carbonic, by heat they are in part diſſipated; and what cannot be driven off by heat, the reſiduum Dr. Thomſon calls *the calx's oxygen:* But what authority has he for it? Why, none. When, by mere heat, the yellow oxyde is obtained from the nitrates, he is obliged to ſay, "But "the loſs of weight ſuſtained, is uſually greater than it "ought to be." Thus, their experiments are conſtantly at variance with their theory!

Mr. Prouſt, who, in ſpite of his attachment to the French theory, is certainly a very ingenious chemiſt,—ſays that he has made an oxygen of lead, with only five per cent. of oxygen, while he ſuppoſes the yellow contains *ten* per cent. This Dr. Thomſon endeavours to account for.

The great Scheele has diſcovered what is called a particular oxyde. The Doctor ſays, "When diluted nitric "acid is poured upon red lead, the greater part of the "oxyde is diſſolved; but a brown powder remains be-"hind, which is not acted upon by the acid." This brown powder is ſuppoſed to contain twenty per cent. of oxygen. Red lead is clearly formed by the proceſs of calcination, the fire uniting to the acid in the calx; and that part which has received the greateſt quantity of fire, is not ſoluble in the diluted nitric acid. It was for this reaſon that J. L. Gay Lupac, (ſee *Nicholſon's Journal for Auguſt)* found that the red calx of iron is ſooner precipitated from its ſolution in the acids than the black calx. But all this, according to *their* theory, ought to have been different; for Mr. Lavoiſier's theory ſays, before a metal will diſſolve in acids it muſt be an oxyde; but here the calces moſt oxygenated appears the leaſt ſoluble in acids. But my theory ſays, that the more a calx is calcined, the more ſoluble it is in acids: as gold, it requires the powerful action of two acids; but even the acetous acid will diſſolve it when it is a calx.——Therefore, clearly, we have a right to ſuppoſe that the brown oxyde of lead, and the red oxyde of iron, are calces the leaſt calcined; that is, more inclining to the metallic ſtate:--And as the metallic ſtate is a thorough ſaturation of fixed fire, therefore theſe calces poſſeſs a ſmall ſaturation of fire; ſo much ſo, that when heat is applied to the brown oxyde, it gives out 20 per cent. of a fire air, which they abſurdly call oxygen gas. The nitric acid, together with the carbonic acid in the lead, are expelled by the heat, ſo highly neutralized and ſaturated with this fire as to produce this air. But if the calces were not expoſed to a long and repeated action of fire for forty-eight hours, theſe acids would be expelled by the heat, as the nitric acid and water, and as the fixed air and water. But ſometimes theſe acids get ſo ſlight a ſaturation of fire as to form only azote.—(See page 11.)

They have all along taught that the lead and iron in theſe high calcinations, or long expoſure to the action of fire, were in receiving oxygen gas from the atmoſphere. But the oxygen gas received from the lead is a different air from the atmoſphere, as we have ſhewn in page 218.—Let us, however, ſee how it is made[*]; and then, reader, thou wilt be qualified to judge for thyſelf.

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[*] Dr. Watſon's third volume of *Chemical Eſſays*, contains the following intereſting particulars.——"The furnace is very like a baker's oven; its vaulted roof is not at a great diſtance from the bottom or

Now, reader, ſeriouſly reflect.—Here is the yellow calx of lead, which they allow is thoroughly calcined; and then, the Doctor ſays, “it is thrown back again into “the furnace, and being conſtantly ſtirred, ſo that all its “parts may be expoſed to the action of the flame of the “pit-coal, in about 48 hours it becomes *red lead*, and “is taken out for uſe.”——Again: conſider the *make* of

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floor; on each ſide of the furnace there are two party-walls, riſing from the floor of the furnace, but not reaching to the roof. Into the intervals between theſe walls and the ſides of the furnace, the pit-coal is put,—the flame of which being drawn over the party-walls, and ſtriking upon the roof, is from thence reflected down upon the lead, which is placed in a cavity at the bottom, by which means the lead is ſoon melted. The ſurface of melted lead, when expoſed to the open air, inſtantly becomes covered with a duſky pellicle; and this pellicle being removed, another is formed; and thus, by removing the pellicle as faſt as it forms, the greateſt part of the lead is changed into a yellowiſh green powder. This yellowiſh powder is then ground very fine in a mill, and being waſhed, in order to ſeparate it from ſuch parts of the lead as are ſtill in their metallic ſtate, it becomes of an uniform yellow colour; and when it is dried to a proper conſiſtency, it is thrown back again into the furnace, and being conſtantly ſtirred, ſo that all its parts may be expoſed to the action of the flame of the pit-coal, in about 48 hours it becomes *red lead*, and is taken out for uſe.

“The colour of the red lead admits ſome variety, which is occaſioned by the different degrees of heat. If the heat is too ſmall, inſtead of red it is yellow or orange-coloured; if it is too great, the red colour is changed into a dirty white: between theſe two extremes it is ſubject to ſome diverſity of ſhades of red, which cannot well be noticed or deſcribed, except by thoſe who are engaged in the making of it.

“It has been aſſerted that the reverberation of the flame and ſmoke upon the ſurface of the lead, is not a neceſſary circumſtance in giving it a red colour, but that it will acquire this colour by a long calcination, without coming in contact with the flame. The truth of this aſſertion I think may be doubted. I have more than once calcined lead for above 60 hours, without ſuffering the flame of the fire to touch it during any the furnace; and it muſt clearly appear to thee that its conſtruction is ſolely for the purpoſe of expoſing the yellow calx *to the flame* of the *pit-coal*, and not to the *air*, otherwiſe this furnace would be a bad contrivance for that purpoſe. The furnace is ſo conſtructed, that the flame is reverberated upon the metal; and what is flame, but the carbon of the coal, turned into actual fire?—— It follows, then, that the air in the furnace muſt be turned into fixed air; for the lead turns into the yellow calx the beſt in the open air, (if it be heated enough), where it has free acceſs to it in a pure ſtate. But to be turned into red lead, it requires the flame to be REVERBERATED UPON IT. Hence it is moſt manifeſt how neceſſary the flame is in forming red lead, for without it there is only a calx of 'a dirty red' formed. Dr. Watſon ſays, (page 326,) "The litharge which is firſt formed, is whitiſh; that which experiences a greater degree of heat, is red." Now can any facts declare more forcibly, that *the calx of lead acquires its red colour from fire?*

But only attend to the proceſs of making colcothar, or the red calx of iron: what an immenſely ſtrong heat is required! (See the proceſs in many of the old Pharmacopœias.) Dr. Thomſon ſays, "The red oxyde of lead does not ſeem capable of combining with acids. Many acids indeed act upon it, but they always begin by re-

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part of the proceſs; but by this method I could never obtain any thing better than a dirty red, reſembling the red of brickduſt, which is very different from the colour of red lead; and even this dirty red was changed into a yellow colour, by augmenting the degree of heat with which the lead had been calcined. The method of making red lead is very well underſtood in England and Holland, but not in France; and the French workmen are of opinion, that it cannot be made by the flame of wood fires.'

ducing it to the ſtate of yellow oxyde. The fixed alkalies do not alter its colour, but they gradually diſſolve it. From this ſolution it is thrown down always in the ſtate of yellow oxyde: Hence it muſt loſe oxygen during the ſolution."

Now, I would aſk Dr. Thomſon this queſtion — *If the acids are more oxygenated after diſſolving it?* (as they ought to be, according to the French theory.) And alſo, *What becomes of the oxygen, when the alkalies diſſolve it?*— Has he ſagacity to find it out?

He now treats of letharge, which he ſays contains four per cent. of fixed air. Let us conſider what letharge is. It is the calx of lead, formed by a ſtronger heat, ſo as that the calx gets in part vitrified.—Dr. Watſon, while ſpeaking of letharge, which is made by lead being calcined in a very ſtrong heat, in order to get the ſilver from it, ſays, "The letharge which is firſt formed, is "whitiſh; that which experiences a greater degree of "heat, is red." The veſſel in which this calcination is conducted, the Doctor ſays, "is of a ſhallow form, that "a large ſurface of the melted maſs may be expoſed to "the air." Very different from the furnace which is to make the minium! In one caſe the deſign is to expoſe the ſurface of the lead to the air, which is to make the letharge; the other, to make the minium, is, clearly, to expoſe the lead to the flame. *Juſt the reverſe of what this wonderful French theory teaches!* Minium, after it is made, may be eaſily converted into letharge; and without parting with its air, by being expoſed to a ſtrong heat its fixed fire is burnt or ſet free, leaving its fixed air it was united to in the letharge. Need we be ſurpriſed at this, when the electric ſpark, taken in the atmoſpheric air, will burn or ſet free its fixed fire, leaving its rich part, its oxygen gas, in the ſtate of fixed air.

But I ſhould think my candid reader would declare it to be unneceſſary for me to ſay any more. Nevertheleſs, I ſhall quote a paſſage from my *Thoughts on Air*, publiſhed in 1785*. But our theoriſts *then* would not liſten to the voice of reaſon.

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* "Here we ſee the quieſcent fire of the air, and the quieſcent fire of the lime, producing the ſame effect; but the latter being in a more looſe active ſtate, produces more active effects. If the volatile ſalts of the bile are expoſed to the air, they will attract the quieſcent fire of the air, and become cauſtic. If volatile ſalts be prepared with quick lime, they attract the fire of the quick lime, and likewiſe become cauſtic. The calces of metals, minium for inſtance, which is well known to be the calx of lead with a quantity of fire in it, which it received in its calcination; by mixing it with the ſal ammoniac it will decompound the ſalt, ſetting free the volatile alkali, which is owing to the fire of the minium having a great degree of attraction for the volatile alkali, and aided by the minium's attraction for the marine acid; ſo that both acting together, decompound the ſalt. It could not be altogether from the minium having a ſuperior attraction for the marine acid; for the volatile alkali will decompound a ſolution of lead in the marine acid.

"The calx of lead, formed by the fire into either minium or litharge, will decompound the ſal ammoniac. But if either of them be diſſolved in an acid, and then precipitated by the volatile alkali, the alkali will unite with the acid, and the fixed air of the alkali will unite with the calx. But if we apply this calx, which has now got fixed air, inſtead of the fire, (which the acid robbed it of when firſt diſſolved in it,) it will not decompound the ſal ammoniac. Therefore this clearly proves, that when the calx is united with a quantity of fire, the volatile alkali of the ammoniac will attract it ſo as to part with its acid; but when the calx is united with fixed air inſtead of fire, the alkali of the ammoniac will not have ſo ſtrong an attraction for it as to part with its acid.

"The calces of metals poſſeſs a quantity of fire—yet, by expoſure to fixed air, the air will not be attracted by the calces ſo as to diſlodge the fire; but a ſtronger acid will diſlodge it.

Now here, beyond all doubt, the minium juſt acts the ſame part as lime does—a well-known fact: and even you, Dr. Thomſon, at length admits that the lime contains light. And you ſay, that "Fourcroy, on the authority of Vanquelin, affirms, that ſulphur takes fire when triturated with brown oxyde of lead;" at the ſame time you muſt *know*, that ſulphur takes fire when triturated with lime. Therefore I think I may be allowed to ſay, that when the fixed alkalies diſſolve the brown oxyde of lead, a double attraction takes place; the alkalies attract the fire, and the oxydes of lead attract the fixed air of the alkali; which may be got from them again by fire, expoſing the yellow oxydes to heat. Ye bigotted chemiſts! can you deſire a train of more rational reaſoning than this,—which is without a chaſm or a flaw! Be your prejudices ever ſo ſtrong, *you muſt open your eyes*.

It is really a curious fact in the hiſtory of human nature, how men's ſenſes can become ſo abjectly depraved as to be ſubſervient to ſuch groſs hypotheſes as they are compelled to have recourſe to in their explanations.—— Mr. Morveau, that wild theoriſt, whom Mr. Kirwan compliments with being *a phalanx himſelf*, at the ſame time that he calls upon him emphatically to clear up the difficulties, and to give *intelligible* explanations (otherwiſe

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"Though the calces have an attraction for fixed air, viz. when a ſolution of them is decompounded by the volatile alkali; and although the atmoſphere is capable of giving fixed air to bodies, viz. in quick-lime, when expoſed to it; yet the calces, when expoſed to the atmoſphere, are not decompounded.

"As quick-lime will roaſt an egg,—therefore, agreeably to Doctor Black's theory, it muſt do it by attracting its fixed air; but then a common fire muſt likewiſe roaſt it, from expelling its fixed air,—as the lime and fire equally roaſt it alike."

they are no explanations) of certain phenomena that are hoſtile to *their* theory: this ſagacious gentleman obſerves, that thoſe who would breathe pure oxygen, ſhould be provided with a bottle filled with the oxmuriatic acid, upon opening which, they will be ſurrounded with this pure atmoſphere. In the 28th volume of the Monthly Review, there appears the following very juſt obſervation upon *Mr. Rupp's paper on bleaching*. "The importance of this memoir to the manufacturer is hardly to be calculated. At the period of the firſt introduction of the new mode of bleaching, it was found impoſſible, on account of the ſuffocating vapours of the oxmuriatic acid, to make uſe of it in open veſſels."—Ecce *their abſurdities and contradictions!* Is it not aſtoniſhing, that theſe men ever could ſuppoſe that this moſt acrid acid could poſſeſs oxygen!............But, reader, I think I have taught thee to be ſurpriſed at nothing. The hyper-oxmuriatic acid will not bleach in the leaſt, but the fixed alkalies are the beſt bodies for it. Here the one, according to them, poſſeſſes more oxygen than any bodies in nature, but the other not a particle of it. What ſhall we ſay to this, ye *SOLOMONs!* Go, and learn your A, B, C. of common ſenſe.

What ſhall I ſay to future ages, while I acknowledge I have done that which they will condemn me for. To the preſent age I ſhall make no apology; their baſe and unjuſt conduct has extorted from me the ſeverity of reflection. Some time ago I was very ill, in which ſituation I was attacked by a train of reflections upon the conduct of the world in reſpect to me and my writings—not the leaſt part of which was, that in many caſes they had even ſtolen my ideas, without making the ſmalleſt acknowledgments—nay, without ever ſuggeſting or hinting at my name: and as mine adverſaries had not ſcrupled thus injuriouſly to treat me *when living*, what would they not attempt when I was *dead?* Under a ſudden impulſe of chagrin, I collected together ſome important manuſcripts and committed them to the flames! I ſoon repented my raſhneſs, but it was too late; the deed was done! Oppreſſed with ſickneſs, and diſguſted with the villainny of mankind, I was determined that they ſhould no longer raiſe their fame upon my labours. SCIENCE! I addreſs myſelf to Thee. Beneath thy ſacred ſhade I hope to find protection and repoſe. Thy faithful diſciples (a long ſucceſſion!), while coolly reflecting upon the cruel and unjuſt treatment I have received, will, I hope, forgive the deed, though they ſhould curſe the authors of it[*]!

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[*] I do not know a greater grievance in literature, nor one which demands redreſs more, than the diſpoſition that manifeſts itſelf among literary characters, of giving unlimited latitude to thoſe who ſtile themſelves Reviewers and Journaliſts. They are a real bane to Society, being made the *tools* of deſigning men, either to eſtabliſh or depreſs a ſyſtem—to ſuppreſs or ſell a publication. To gratify the curioſity of future generations, when theſe peſts may ceaſe to exiſt, I ſhall here give a ſpecimen how their buſineſs is conducted, by citing an advertiſement which has been repeated in all the newſpapers and periodical productions for upwards of theſe three laſt months.

"TO REVIEWERS.

"WANTED a good Hand, to write in an old-eſtabliſhed Review,
"one who can undertake the departments of experimental philoſophy,
" poetry, claſſical criticiſm, metaphyſics, mechanics, and
"the oriental languages. A perſon uſed to a ſarcaſtic and ironical
" vein of compoſition will be preferred. To ſave trouble,
"the terms are Two Guineas and a Half per printed ſheet, quotations
" included. Concealment is warranted, and a competent

I flatter myſelf that I have fully ſhewn the true principles upon which VOLTA's *Pile* acts; that I have accurately and clearly elucidated all the phenomena upon the principles of my philoſophy, bringing them as the direct proofs of each other's validity. I did not publiſh my

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"guarantee will be given againſt proſecutions for libels. Addreſs, "poſt-paid, to Ariſtarchus, at the Bar of the Chapter Coffee-"houſe, Paternoſter-row.'

"N. B. Thoſe Books which it is neceſſary the Reviewer ſhould ſee, "are expected to be returned clean and uncut, or the value will "be deducted."

Now, ſuffer me to aſk every modeſt and candid friend to Truth, every well-wiſher to the honour of Science, what he thinks of this body of men? Here a man is advertiſed for, with a head fitting an Encyclopedia, *to underſtand every thing*, ſo as to be qualified to criticiſe the moſt learned works. Bnt whether he is thus accompliſhed, or not, it is deſireable, it ſeems, that he be "uſed to a ſarcaſtic and ironical vein of compoſition." This is the grand ſecret of the art; as to underſtanding the ſubject, *that* is very immaterial. To pleaſe the bulk of readers, who are as ignorant as himſelf, he muſt have the 'ſarcaſtic vein of compoſition.' This will *tickle* them where he cannot inform them; in ſhort, it is worth a magazine of learning. Yet, he muſt not only be the moſt learned man in the world, embracing ſubjects which have no connection with each other, but he muſt carry this wonderful erudition *in his noſe;* for he is not permitted to cut open the leaves, but he muſt, by the application of this extraordinary noſe, *ſcent* it out, even as a hound does its proper game.—But then he is to receive his *cue* from his employers how he is to behave to the game—whether to run it down at once, or to allow it ſome ſcope of action; whether to tear it in pieces, or ſuffer it to eſcape.——If Juſtice, Truth, and Science are reſpectable in the eyes of the world, I doubt not that many will concur in giving theſe mercenary Blood-Hounds ſome pointed marks of their diſapprobation, which I have a juſt right to expect, from their behaviour to me.——Above all, I could wiſh to ſee a Review eſtabliſhed upon fair and liberal principles, ſuperior to the *craft* of authors, bookſellers, or any ſet of men whatever. No independent or ingenious writer would, to ſuch a publication, refuſe his aſſiſtance.—This Reviewer is to have the heads of Cerberus, with all their malignity.

opinions haſtily, well knowing that when I had clearly elucidated the whole, the great body of our Engliſh philoſophers, who were the foremoſt in proſecuting experiments with the pile, would, upon ſuch elucidation, immediately abandon the purſuit, and ſtrive to bury the whole in oblivion, under a conviction that it tended to cut up the French theory by the roots. It is for this reaſon, that the unprincipled combination I am ſpeaking of, by every mean and cunning artiſice, endeavour to

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In this place I think it not unſeaſonable to take a ſlight retroſpect of the behaviour of *theſe gentlemen* to me, within theſe four-and twenty years,—during which time they have been buſy in loading my writings with the greateſt contempt and abuſe, calling in the aid of ſalſe aſſertions; and when I convicted them of ſlanders and lies,—pho! they had no regard to it, knowing that where I had one reader, they had ſome hundreds. Not to go back far, I ſhall juſt cite ſome of their late criticiſms upon my Letter to Mr. Cavendiſh. In the Monthly Review it was obſerved that my book required no inveſtigation, for the French theory was then completely eſtabliſhed and allowed by every one. Behold, in their very next Review, within a few months upon Mr. Humboldt's publication, on ſome points they recognized the very ſentiments and opinions that my book taught them, without either naming it or me. In the 30th volume of the abovementioned work, page 491, the following obſervations occur:—"It was unfortunate, it was raſh and illogical, to aſſume that all the gaſſes are derived from ſolid baſes. The great Lavoiſier was evidently miſled by the notion of *latent heat;* an hypotheſis originally founded in paralogiſm, and which has materially impeded the progreſs of ſcience:—but is there not a radical diſtinction between vapours and permanent gaſſes? And even granting that various additions of heat are capable of changing any ſolid ſucceſſively into the liquid and the aeriform ſtate, what reaſon is there for maintaining the converſe of the propoſition? The adherents of phlogiſton have been accuſed, and with reaſon, of creating an imaginary exiſtence: but are their opponents altogether exempt from ſimilar reproach? What are oxygen, hydrogen, azote, but *êtres de ſcience,* beings not cognizable by the ſenſes, and not demonſtrated by their perceived effects?"

ſtifle the ſpirit of enquiry. But how our Engliſh experimentaliſts could drop their inveſtigations ſo ſuddenly, and take no notice of mine, after being reſpectfully invited to do ſo, is indeed unaccountable—only, it is of a piece with that unexampled and ſhameful behaviour which I have experienced from them all — a behaviour that will one day be ſeen in its proper colours, when a ſuitable puniſhment will fall upon this nefarious junto, whoſe aim invariably has been to ſuppreſs Reaſon, Common Senſe and Truth, at the expence of Candour, Honour and Juſtice.

In conſequence of having clearly ſhewn, that in Volta's *Pile* the electrical fluid paſſed from the ſilver wire to the zinc, my opponents have lately been endeavouring to prove that it paſſes from the zinc to the ſilver wire. Let us inveſtigate it:—in doing which I will attempt a full inveſtigation, and more *minutely* than I have done in my former tract.

Alſo, as if they had in their eye my writings, they ſay, " It may be doubted even, notwithſtanding all the declamation on that ſubject, whether the accumulation of iſolated facts tends really to the advancement of ſcience. He, whoſe genius ſeizes the threads of the connecting analogies, generally founds his theory on properties diſcovered by himſelf, or on ſuch as are familiarly known. It is the occupation of the envious to rake into neglected volumes, that they may detract from eminent merit."

I poſitively aver, that I have perfectly fulfilled that opinion. Before even I was acquainted with their experiments, I clearly elucidated all the phenomena of the atmoſphere; its formation, its uſes, compoſition and principles; all which have been verified by the future experiments of philoſophers. So that, from the firſt point at which I ſet out, I have not deviated in the leaſt from my firſt principles,—whilſt their's have been as changeable as the wind! And into what a labyrinth of errors have they been ſeduced at laſt!

I there ſhewed that metallic bodies attracted the electric fluid, accordingly as they were ſaturated with fixed fire, and that they attract free fire juſt the ſame: therefore that zinc, and ſuch metals as have naturally a high ſaturation of phlogiſton, (that is, fixed fire) will attract the electrical fire. That the zinc attracts a ſaturation of it from the atmoſphere and contiguous bodies; but when a metal, (ſilver, for inſtance) is brought in contact with it, (as all metals have a higher ſaturation of the electrical fire than moſt bodies,) the zinc will attract a part of the ſilver's ſaturation, ſo that it will become *plus*, and the ſilver *minus* to each other; and they will ſhew this change to an electrometer. Moreover, when there is a regular Volta's *Pile* built of theſe two bodies, the zinc will be *plus* and the ſilver *minus;* but when the two ends are united by wires, ſo that the pile begins to work, the electrical ſtates of the metals are reverſed, — as I ſhall, I hope, indiſputably prove to every man of common ſenſe, reaſon, juſtice and candour, though I will not ſay, TO

The British Critic, in his firſt efforts at criticiſm when my opinions were the ſubject, took up the matter as ſeriouſly as though he really thought I was wrong; and began to give ſome abſurd reaſons, in contradiction to my ſyſtem: but I anſwered him ſo pertinently, ſhewing the extreme futility of his reaſonings, that, with the true ſagacity of a *Britiſh Critic*, he ſoon diſcovered how ridiculouſly he was ſituated, and then adopted the ſame line of conduct that had been purſued by the other Reviewers — namely, *condemning in a lump, in a corner of their catalogue.*——As to the Editor of the Critical Review, his conduct appeared ſo juſtly reprehenſible, that another writer undertook to check his arrogance, which he did effectually, and gave him *the lie direct*, in reſpect to my writings; which affront he prudently pocketed, conſoling himſelf with having, from the High Tribunal Chair, condemned a ſyſtem which he could not refute.——But theſe are trifles to what I can relate!

the members of this ſhameful combination, for they have nothing to do with common ſenſe, reaſon, juſtice and candour,—no, nor even with humanity: their behaviour to me has been inhuman in the extreme!

We will ſuppoſe the zinc at the top and the ſilver at the bottom of the pile; the zinc will be found *plus*, and the ſilver *minus*. I have ſhewn that an electrical body, electrified *plus*, will repel the electrical fire of a body electrified *minus;* that is, the electrical fire acts from *repulſion;* therefore the more this fire is concentrated, the more the zinc's electrical fire *repels;* therefore its repulſion muſt be ſtronger than the ſilver's, and, in conſequence, move it into the diſk, which I ſuppoſe to be formed of water, or any other body*. But then it is ſtopped from moving it much, as the next body beyond the diſk is zinc; and this zinc repels it back again into the ſilver. This identical effect is produced through each pair, and therefore through the whole pile; ſo that the pile has a ſtrong electrical tenſion, if I may ſo expreſs it. But when a communication is formed between the zinc at the top and the ſilver at the bottom, the zinc then repels the ſilver's electrical fire into the water, and then into the remoter portions of the zinc, as its fire was not ſtopped by its next neighbour, the ſilver; ſo that each pair's electrical fire will begin to move, upon which enſues a regular motion of the electrical fire, downwards. For as the pile conſiſted of all theſe electrical metals, and as the zinc repelled moſt, and the ſilver leaſt, the conſequence is, that a wire touching the bottom of the pile,

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* What I mean here by being electrified *plus* and *minus*, is comparatively; that is, they both have a great proportion of electricity, but the zinc the moſt.

(which is ſilver,) the electrical fire will be forced into it; and then, this ſame wire no ſooner communicates with the zinc-plate at the top of the pile, than its electrical fire is forced into the zinc-plate: ſo that a regular and active ſtream of this electrical fire will flow in this circle. ═══ Thus far my theory ſpoke; but let us ſee how it is verified by ſubſequent facts.

Dr. Van Marum, that firſt and ableſt electrical experimenter, found that the pile was *plus* at the zinc, and *minus* at the ſilver end: but when it began to work, that is, when a communication was made between the two ends of the pile, the zinc throughout was *minus*, and the ſilver *plus*—though, previous to ſuch communication it was reverſed.

Now we will accurately inveſtigate the pile's action, when it is working.—As there is now a communication, the zinc being at the top, its electrical fire will be no longer hindered in repelling the ſilver's electrical fire, as the zinc in the next pair no longer repels the ſilver's electrical fire; ſo that there is a regular motion downwards, accompliſhed by the obſtruction of the current being removed, and which again is owing to the ſilver at the bottom of the pile having its electrical fire put in motion.

Here then is the electrical fire, paſſing through metals which are *well known* to be the beſt conductors of electricity; but it is not an uniform ſtream through metals, but partly through water, which is equally *as well known* not to be ſo good a conductor as the metals *: therefore this water will induce a ſmall retardation of the current. And it is equally well known, that when a current is obſtructed, it accumulates at the point of obſtruction: thus it is *at the ſilver*, and therefore it becomes *plus*. But the obſtruction is ſoon overcome; for, upon the leaſt accumulation, the water attracts it; — in the zinc, nevertheleſs, it runs ſo eaſily and rapidly through it *into the ſilver*, as to ſuperinduce the *minus* ſtate; and bodies communicating with it, will ſhew *minus electricity*. Imagine two rivers, united at the ſame angles, (as the pile, and the battery to be charged;) the one a large body, the other a ſmaller; if there is no current, there will be an accumulation of water in both: but upon the great water rapidly moving, its own accumulation will be leſſened; as will alſo the ſmaller river, proportionably. It is preciſely the ſame caſe with the pile and the electrical battery.

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* I have marked with Italics both theſe facts, as being *well known*,—though we may poſſibly find, in the oppoſition which is raiſed to my theory, both theſe "well-known" facts contradicted. I can only ſay, *Riſum teneatis amici!*

This, I think, muſt appear clear: but it has been diſputed, and the contrary poſition maintained, namely, that the electrical fluid flows from the zinc wire, through the water, and thence to the ſilver wire. In ſupporting this opinion, however, they are obliged to contradict one of the moſt firmly-eſtabliſhed facts in electricity,—viz. that metals are the beſt conductors of electricity. For, as they allow that the oxidation of the zinc, upon its ſurface, ſets looſe a great quantity of electricity, they ſay that this electricity does not paſs through the metallic zinc directly, but through the water. A Mr. Cuthbertſon ſupports this opinion; but as weak minds muſt have always ſome plauſible reaſon, however ſhallow it be, he alledges that it is owing to the electricity of the zinc and ſilver balancing one another. Undoubtedly, it was a *bright idea* that the weaker electricity repels the ſtronger, ſo as to force it to paſs through an imperfect conductor! and yet I muſt apologize for declining all anſwer to *ſuch kind of reaſoning*. But who could believe that they ſhould perſiſt in ſuch abſurdities, when they are obliged to confeſs (in the words of Mr. Davy) that "In all compound circles of the firſt claſs, the moſt oxidable part of all the metallic plates evolves the influence, appearing as poſitive electricity; whilſt the leaſt part ſeems negatively electrified. In the ſecond claſs of circles it is, however, probable that this order is reverſed."—*Philoſoph. Mag*. vol. xi. page 335.

But there is yet another champion of error, a Doctor Boſtock, who, if poſſible, aſſigns a more ſilly reaſon than any who have gone before him. In *Nicholſon's Journal for October*, 1802, he ſays, "While the electric matter is diſengaged from the oxidating ſurface, it ſeems impoſſible to imagine that it can be received by the ſame ſurface from which it has been diſcharged, and which is continually diſcharging freſh quantities of the fluid." —So much for this gentleman's *impoſſibility*. But his whole paper is replete with abſurdity! Here are no difficulties; the author only requiring the reader civilly to grant him certain *poſtulata*, without paying any regard whatever to the different facts or phenomena.—The entire production is too ridiculous to be mentioned,—only I muſt give the reaſons which have been brought in favour of this opinion. Let us ſee what is the calcination of the zinc: It forms, firſt, the thinneſt coat of calcination, ſetting looſe the zinc's electrical fire; then, as it is ſet looſe, it has the water to attract it, on one hand,—and the metallic zinc, upon the other. But, as the calcination gets deeper, here the ſcene is juſt reverſed to what the Doctor ſays.—The next coat that is calcined, its electrical fire, ſet looſe, has not the water contiguous to it; but it has the firſt coat of the calcination to paſs through, before it can approach the water; and as it gets deeper, theſe coats are accumulated. Here then the Doctor muſt ſay, as an improvement upon Mr. Cuthbertſon's reaſoning, that the calces (or oxides) of metals, attract the electrical fire better than their metals.—*What wonderful diſcoveries,* Mr. Nicholſon, *your boaſted Journal gives birth to!* infinitely ſuperior to any of *my* old-faſhioned facts, which it would be high treaſon againſt the *majeſty* of this favourite French theory to countenance, or even to name. *What does ſuch conduct deſerve?*...... But theſe traders in philoſopy *deal* not in truth, and they have no idea of juſtice or propriety. It would ſeem as though their philoſophy drew along with it, or was the inſtrument of occaſioning, great acts of injuſtice: for honour, juſtice, ſcience and truth, are of no moment; gain is the object: they *muſt* pleaſe the combination.

But Dr. Wollaſton has made ſome experiments on the Galvanic pile, the principal of his ideas and experiments being taken from my publication. What induces me, in this place, to notice his paper, is, that one experiment is ſaid to ſhew that it is the *minus* electricity that gives the inflammable air: as ſuch, it is pompouſly quoted by Mr. Nicholſon in his Journal. The experiment is this; —in paſſing the common electricity through a ſolution of copper, the *minus* wire was covered with copper. But is it not known to every electrician, that the electricity of the common machine produces upon fluids a violent motion; that water, in paſſing through a tube, though it only paſſes by drops, yet if it is electrified by the common machine, it will flow in a rapid ſtream*. Now, the electrical fluid, acting upon this ſolution, would impel it from the *plus* wire to the *minus* one with great force; ſo that the copper which received the inflammable gas of the *plus* wire, would be forced upon the *minus* wire.

But if you will place betwixt the two wires any metallic body, ſo as to hinder the current,—and make a diviſion of the ſolution betwixt the wires, you will find this dividing metallic body impregnated with copper on the *plus* ſide, and not on the *minus*.

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* In my Theory of Electricity, (which the public has long been in poſſeſſion of,) I demonſtrated the abſurdity of the theory of two fluids, the *vitrious* and *reſinous*. The idea of two *igneous fluids* attracting each other, (fire *attracting* fire,) that I abandoned it at once. But I remember making an experiment that was directly deciſive, which I did not then communicate to the public, becauſe I judged all reaſoning upon ſo abſurd an opinion to be unneceſſary and even ridiculous. This experiment conſiſted of taking two very ſmall tubes of glaſs, and filling them with water; then placing, juſt within one end of the tubes, a conductor, the ſame in both the tubes. [The tubes were of a very ſmall bore at each end, but more enlarged at the middle, which would hold near an ounce of water. When filled, the water ſlowly dropped from each end of the tubes.]——After this, theſe two tubes were placed within a very ſmall diſtance of each other, when a wire was introduced into each, ſo as to form betwixt them an electrical communication. At their oppoſite ends were placed two wires, which communicated with the *plus* and *minus* parts of a common electrical machine. Now I argued, that upon electrifying the tubes, by the current of the water they would clearly aſcercertain whether there were two electrical currents or only one. Upon the machine working, the water rapidly flowed from the tube of the *plus* wire, at the end the fartheſt from the conducting wire; but at the other, the water flowed rapidly out at the end neareſt the receiving wire; ſo that clearly there was the motion only of one fluid. I have tried the ſame experiment with the Galvanic fluid, and the ſame was the reſult.

But our French theoriſts have been greatly ſurpriſed, that the electrical fluid from the common machine does not in the leaſt impede the fluid in the pile, which it certainly ought, and *would*, if the two currents were oppoſed to each other; but, in *their* erroneous theory, on an experiment made by themſelves, the fluids are made to flow both in the ſame direction, owing purely to their miſtaking the direction of the current in the pile, it paſſing from the ſilver wire to the zinc wire, and NOT *vice verſa.* But, to put the matter out of diſpute, apply the metallic wire of a conductor from the common machine, to the zinc of the pile next the water of the diſk, and then notice which way its electricity flows; it will decide the queſtion at once, and effectually ſilence all cavils.—Will it flow through the water to the ſilver, think you, *ye wiſe theoriſts?* NO—it will flow through the zinc to the ſilver contiguous to it, the ſame way as flows the fluid of the piles.

But, after the clear explanation of the whole proceſs, which I communicated to the public in January 1801, it appears hard that I ſhould have theſe abſurdities to combat afreſh. But this will ever be the caſe, until the public ſhall be prevailed upon to reſcue Science from the gripe of Uſurpation, doing credit (at the ſame time) to their own judgment and feelings.

Mr. Davy, though a warm and determined ſupporter of the French theory, after my publication on VOLTA's *Pile*, was conſtrained to ſay, (in his "View of Galvaniſm," delivered at the Theatre of the Inſtitute,)—"The "year that is juſt paſt will long be diſtinguiſhed in the "hiſtory of ſcience; ſeldom has phyſical inveſtigation "been purſued with greater ardour; and if new facts, "by being ſometimes inſulated and incapable of application to eſtabliſhed theories, have perplexed the public "mind, yet they have at the ſame time been uſeful to it, "by producing a habit of rational and active ſcepticiſm, "which cannot fail of becoming, at a future period, "the parent of truth*."

Mr. Davy, in his experiments, clearly ſhews that the Galvanic fluid in paſſing through charcoal, neither forms hydro-carbonate nor fixed air: *(See Nicholſon's Journal.)* —But, clearly to aſcertain how fixed air is formed in the Galvanic proceſſes, I took a pile, formed of metallic bodies, which had neither zinc nor iron in it†, but made of ſilver and copper, and the diſks moiſtened with ſea ſalt; the pile's action was incloſed in a quantity of pure air, which was paſſed through lime-water, and, as it was imbibed by the proceſs, more pure air (which alſo paſſed through lime-water) was conſtantly admitted. Upon letting it act for ſome time, the pure air diſappeared, and the marine ſalt had loſt the marine acid, and was impregnated with fixed air.

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* Mr. Davy, in the courſe of the Lectures above quoted, makes alſo the following conceſſions: "The general chemical agency of Galvaniſm is at preſent involved in obſcurity. The facts relating to the ſeparate production of oxygen and hydrogen, acid and alkali, in water, are totally incommenſurable with the uſually received theory of chemiſtry. And, even ſuppoſing that the appearance of the two laſt of theſe bodies is connected with the preſence of atmoſpheric air, it is ſtill extremely difficult to conceive, that either oxygen or hydrogen can paſs in an inviſible form through fluids or organic bodies. Common phyſical facts preſent us with no analogies ſufficiently diſtinct to enable us to reaſon on this ſubject, and the elucidation of it will probably be connected with perfectly new views of corpuſcular action."

† Mr. Prouſt has clearly proved, that there is no carbon in zinc.—In addition to which, Dr. Prieſtley has demonſtrated, by various experiments, that there is no plumbago in the inflammable air from iron.—See his laſt pamphlet, called *The Doctrine of Phlogiſton*, page 62.

Now, I deſire that the above experiment with the Galvanic pile may be repeated, — which muſt entirely overthrow the idea that has been ſo long received of the formation of fixed air. It could only proceed from the decompoſition of the pure air, its fixed fire being ſet looſe by the combuſtion in the diſks, and there was no charcoal in the proceſs. Alſo it indiſputably proves, that the marine acid is taken up by the electric fluid, and circulates with it in the pile,—as it is found depoſited upon the two wires which formed the communication between the ends of the pile.

Let this combination no longer advance their abſurdities, but put this experiment to the teſt, and hereafter hold their tongue—or elſe I hope the world will join with me, and cry out *SHAME*[*]!

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[*] I have ſhewn long ago, that the pure fixed alkalies will imbibe the pure air of the atmoſphere, turning it into fixed air. — I would have my reader attentive to this fact. Let him paſs the pure air admitted to the pile through lime water, and he will find ſtill the ſame phenomena: the marine ſalt having loſt its acid, will be found to be highly impregnated with fixed air; and the quantity formed will be found to eſtabliſh this important fact, that the combuſtion in the diſks had ſet free the pure air's fixed fire, turning it into fixed air, which was imbibed by the alkali. Our tiny theoriſts will probably alledge, that when I proved that the cauſtic alkali imbibes fixed air from the atmoſphere, it was the fixed air floating looſe in the atmoſphere: But I ſay, there is no fixed air floating looſe in the atmoſphere. After me, Dr. Prieſtley has ſufficiently ſhewn that there is no fixed air floating in the atmoſphere; that when lime, the oxydes of metals, &c. are found to form fixed air, it is owing to the pure air of the atmoſphere being changed to fixed air: See his laſt publication, entitled *The Doctrine of Phlogiſton*, ſection x, page 60.—Alſo, the immortal Scheele ſays, "Empyreal air, (oxygen gas) unites with lime:" Page 160.

As the pile is formed not of a regular contact of metals, but has ſome fluid interpoſed between them; and the electrical fluid paſſing through the diſks (I have fully proved) produces combuſtion, particularly if the air of the atmoſphere has acceſs to the diſks; and in the combuſtion its pure air is turned to fixed air. Our French theoriſts, knowing that this fact cuts up their theory by the roots, are anxious to overthrow it.—When the plates are made large, there is conſiderably more heat produced in the pile, than when they are of ſmaller dimenſions.—I ſhewed clearly, that by enlarging the diameter of the plates, there was a proportionably greater combuſtion induced. The French chemiſts, *(after me)* by enlarging the diameter of their plates, were able to fuſe metals. I alſo ſhewed, that in ſuch degree as the wires which were

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But, to do away all cavil,—Take a pile formed without any zinc or iron in it, and its diſks filled with a ſolution of the cauſtic alkali; let the pure air that is admitted to the pile paſs through lime water, and, upon the action of the pile, the alkali will become mild, becoming ſaturated with fixed air. I formed a pile of this kind, and kept conſtantly ſupplying it with air paſſing through lime-water; under which proceſs the pure air evidently diſappeared, and was found united to the alkali, as fixed air. I intreat that the reader will not take this upon my authority, but that he will try the experiment.

Indeed this effect is nothing new; for Mr. Kirwan, that changeable gentleman, in his Eſſay upon Phlogiſton, (page 52) ſays, "It is impoſſible to deny all credit to thoſe who aſſerted that lime-water was precipitated by taking the electric ſpark in common air, though it did not ſucceed with Mr. Cavendiſh, either from his uſing an inſtrument of different power from that uſed by others, or air phlogiſticated by a different proceſs."—Now, permit me to aſk Mr. Kirwan if he can poſſibly ſuppoſe the electrical ſpark to be charcoal?

Meſſrs. Biot and Caviot ſhew that the Galvanic pile is an excellent Eudeometer, it attracting all the oxygen gas of the atmoſpheric air, which is abſorbed by the alkali in the diſks and formed into fixed air.

placed in the circuit were long and ſmall, they would give air accordingly: that two long wires forming a part of the circuit, the electrical fluid circulating through them without being expoſed to the combuſtion, would form airs accordingly, and leſs heat: that the fixed fire which the electrical fluid took from the bodies of the pile, by paſſing through diſks of a large ſurface, combuſtion would take place accordingly, accompanied with intenſe heat: that when the electrical fluid was confined to long wires of a ſmall diameter, it would rob the wires of their fixed fire; and, meeting with water in its paſſage from one wire to the other, would receive the water of compoſition neceſſary to form airs; and does form airs accordingly. Therefore piles formed of large plates, and piles formed of ſmall plates, the intenſity of the electrical fire would be the ſame; the only difference would be in the heat induced, which is very conſiderable.

Our French theoriſts pretend to account for this immenſe difference, by preſuming that it is owing to the greater velocity in the one than the other. But *their* vague gueſſes and reaſonings, unfortunately, are always at variance with reaſon and common ſenſe:—the reverſe ought to be the caſe;— as the fluids in the diſk would *certainly* impede the paſſage of the fluid, and not accelerate it; as the water on the diſks would *certainly* impede its courſe more than the metals.

But that this was actually the caſe, is proved by Doctor Marum's experiments *.—Here it appears that moiſt paſteboards were neceſſary to this intenſe heat. Now, certainly, the metallic plates being applied together, would circulate the fluid more freely than when the moiſt paſteboard intervened; and this directly accords with my ideas on the ſubject — that the paſteboard, by breaking the circuit, the electrical fluid in this interruption would meet with pure air, to aid the combuſtion ſo producing a greater heat*. In my reaſoning, *Two and two make four:* but *they* heap abſurdity upon abſurdity, (*a monſtrous* PILE!) contradicting the plaineſt and moſt obvious facts. The candour of Dr. Van Marum is truly praiſe-worthy: he admits that *his hypotheſis* (of ſuppoſing it from velocity) *does not account for the phenomena.* But our wiſe theoriſts *here* are not ſo ingenuous: Reaſon or Common Senſe they have nothing to do with. They are true inhabitants of Swift's iſland. Their combination is formed of prejudiced Ignorance, with not a particle of Integrity or Candour. They have a body of Janizaries

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* Mr. Nicholſon, in his account of theſe experiments, ſays, "To ſee how far this velocity depended on the ſize of the pieces of paſteboard employed, two piles were conſtructed, each of twelve pairs of the large plates, in one of which the paſteboard was cut to the ſize of the plates,

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in the other to the diameter of half an inch only. The intenſities of both were the ſame, yet the other effects were much greater in the pile with large paſteboards; for it gave very brilliant ſparks, which began with the fifth pair from the bottom, while the other pile ſcarcely emitted any that were perceptible. On making the paſteboards ſomewhat larger than the metallic plates, the effect was by no means increaſed, but rather diminiſhed. It was found neceſſary likewiſe, that the paſteboards in the large pile ſhould be well wetted.

" There is another apparent anomaly, however, in the effects of piles differing in diameter: — Though the action of the large pile is ſo much more powerful in fuſing metals, not the leaſt difference can be perceived in the ſhocks they give: and this Dr. Van Marum confeſſes is not accounted for by his hypotheſis."—*Journal for March*, 1802—page 179.

* I have always found that as the diſks were expoſed to atmoſpheric air, the heat was accordingly; and if oxygen gas, the heat and ſparks were ſo much more intenſe.

in aid, who are the moſt deſpicable of aſſaſſins! But the day of retribution will aſſuredly come, after that the film has been removed from the public eye, and objects are ſeen in their proper colours: then humiliation and contempt will overtake the enemies of truth — the execrable combination!

I flatter myſelf that I have fully elucidated all the phenomena of Galvaniſm, in the cleareſt manner; and that all the facts that have ſince appeared from the continental experiments*, are perfectly in uniſon with my opinions—That the electrical fire conſiſts of an acid and fire; that metallic bodies have a high ſaturation of this electrical fire, and that ſome metallic bodies poſſeſs it more than others: that when metallic bodies are placed together, the ſaturation of each is diſturbed; and by the aid of wetted bodies a conſtant circulation is formed amongſt them†: That the quantity of the electrical fire is greatly increaſed by the calcination of the metals acting in letting looſe this high ſaturation of their electrical fire: that the bodies employed thoſe bodies that form the diſk; that they even generate the electric fire, and that the combuſtion in the diſks even in ſome meaſure decompounds this electrical fire: that the electrical fire unites to the different bodies in the pile, and carries them in its circuit along the pile: that, with the bodies of the pile, it generates the different airs: that the acid which the electrical fire is formed of, ſeems to be of the nitrous kind; and that it ſhould unite itſelf with the fixed fire of the different bodies of the pile, and receive the water of compoſition, (which I have all along in my different publications *clearly proved* is neceſſary to the forming of airs,) is not at all ſurpriſing; as we have many ſimilar experiments in corroboration of the fact. In ſundry proceſſes, the nitrous acid, with the fixed fire of metals, produced two different kinds of air, viz. inflammable air and (what Dr. Prieſtley calls) *dephlogiſticated nitrous air.*——See my different works, particularly my *Thoughts on Air.* The reader is alſo referred to a Letter to Sir Joſeph Banks, inſerted in the Gentleman's Magazine. Dr. Prieſtley's works likewiſe throw ſome light on the ſubject, where he treats of expoſing the nitrous acid to the metals, and alſo nitrous air to iron; all which experiments muſt be familiar to the chemical reader. Although, to every unprejudiced and enlightened chemiſt, I have ſatisfactorily proved this, —yet our illiberal Engliſh chemiſts never once noticed it. Thoſe deſpicable productions of an unprincipled party,—the Reviews, &c.—*durſt not take notice of it.*

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* After my publication, as I expected, our experimenters ſuddenly dropped their experiments, though they were the firſt and principal ones. As I had elucidated the whole phenomena, it is obvious that if they had gone on they muſt have juſtified my arguments.

† When this fluid paſſes through the bodies of animals, their ſurface in like manner, ſhould be moiſtened with water,—in order to aid the fluid to paſs through them.

In my treatiſe on Galvaniſm, (in 1800) I there gave my theory, which has been fully verified by future experimenters*.—As my treatiſe was drawn up for the periodical prints, of courſe I was as conciſe as poſſible; but none of them *durſt* publiſh it.—*O tempora, O philoſophia!* Can it be conceived, reader, that ſuch behaviour will elucidate ſcience? Will it not obſcure it? . . . . . I truſt, however, that the public will decide impartially between us. . . . . . Even now I have the ſupreme felicity to declare, that my theory of Galvaniſm is perfectly juſt, not having the leaſt occaſion to alter any part of it.

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* "I have found that for a long time paſſing the electric fire thro' metals, (either by the common electrical machine or the Galvanic) particularly thoſe that are the eaſieſt calcinable, even in *vacuo*, that they would loſe a great quantity of their fixed fire, becoming leſs malleable; and when the metal was iron, that it would give, after this operation, leſs inflammable air when calcined with the vitriolic acid and water.——

Some time after my publication, Dr. Wollaſton gave a paper to the Royal Society, *upon Galvaniſm,* which we

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The different metals of the pile having this conſtant circulation of the electric fluid through them, would, in conſequence, take a part of the fixed fire of the metals in the circuit; but particularly thoſe metals moſt calcinable, as the zinc, copper, iron, &c. And alſo as the moſt calcinable metals are moſt expoſed to the influence of this electric fire on one of their ſurfaces, viz. that ſide which forms the diſk, receiving the fire of the circuit; which fire's action is aided by ſalt, acids, water, and atmoſpheric air; all which operating, and ſometimes together, muſt calcine the ſurface of the zinc or metal that is expoſed to their action, which action will be a kind of combuſtion. After this, the electric fire circulates through the body of the zinc plate, and alſo the ſilver plate; it then circulates to the next diſk, and is expoſed again to the action of all the above bodies juſt named,—and ſo on.

"The action of the bodies in the diſk hinders the electric fluid from generating itſelf into air; for as it attracts fire from the metallic bodies, the combuſtion it is expoſed to in the diſks ſets partly looſe its fixed fire, therefore airs are not ſo much formed in the diſks. But the way to form it into airs, is to extend its circulation through the metals; as when the pile is interrupted by two long wires, it there circulates through them for a great ſpace, without being expoſed to the combuſtion of the diſks. And if the two wires (that is, the ſilver wire and the zinc wire) are expoſed to the influence of water in a veſſel, they will generate airs, as has been already ſhewn.

will conſider*. Now theſe experiments draw Galvaniſm into its moſt ſimple form; let us attend to them. — The zinc is acted upon, and not the ſilver; but as ſoon as

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"It was for this reaſon that Mr. Henry, by not having a ſilver wire but only a column of mercury to act for it, gave out no gas. I have found that the ſilver metal, if I may ſo call it, gives out air according to its length, and alſo its breadth, the narrower the better, the electric fire being more concentered to act upon its fixed fire; therefore I have always found that by lengthening and narrowing this metal of the pile, I got inflammable air accordingly. But when the reverſe of this was the caſe, there was only heat and the white calx of the zinc to be found.

"That the ſtrength of the pile's working does not depend upon the calcination of the metals, I aſcertained by excluding the atmoſpheric air, whoſe fire ſo much aids the combuſtion, and only adding a ſmall proportion of the mineral acids to water in the diſks, hindering the combuſtion, the acid at the ſame time aſſiſting in forming the electrical fluid; and the quantity of airs formed, as I have before obſerved, depends upon the quantity of fixed fire that the electric fluid receives in its circulation,—and alſo their quality, being more of oxygen and azote, and leſs inflammable air, when platina wires were uſed: — I found theſe wires formed airs of a leſs concentration of fire than the calcinable metals, as the latter are thoſe which eaſily parted with their fixed fire; and even more acid was depoſited with platina wires. That there is an acid depoſited in theſe proceſſes, I think Mr. Cruickſhanks has fully ſhewn." *Page* 53—56.

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* "EXPERIMENT I. If a piece of zinc and a piece of ſilver have each one extremity immerſed in the ſame veſſel, containing ſulphuric or muriatic acid diluted with a large quantity of water, the zinc is diſſolved, and yields hydrogen gas, by decompoſition of the water: the ſilver, not being acted upon, has no power of decompoſing water; but, whenever the zinc and ſilver are made to touch, or any metallic communication is made between them, hydrogen gas is alſo formed at the ſurface of the ſilver.

"Any other metal beſide zinc, which by aſſiſtance of the acid employed is capable of decompoſing water, will ſucceed equally, if the other wire conſiſts of a metal on which the acid has no effect;

they become Galvanic, (that is, ſo ſoon as they become a chain for the circulation of this electrical fluid) the ſilver then is acted upon. But our *bright* French philoſophers (who moſt prepoſterouſly ſubvert all reaſon, and every fact however well eſtabliſhed) muſt ſay, That when the zinc and ſilver touch each other, the electrical fluid, ſet looſe from the zinc, paſſes through the diluted acid, and ſo reaches the ſilver:—the conſequence to be deduced from which is, that this watery acid muſt attract this fluid ſtronger than the metals! . . . *Abſurd!!!* . . . On the contrary I affirm, in the very teeth of all the *lunatic* French chemiſts, That *metals attract this fluid ſtronger than water.*

It appears from the experiments in the French Inſtitute, that BIOT "has diſcovered that the Galvanic fluid "is tranſmitted with difficulty through water, but that it "eaſily glides over its ſurface; an effect produced alſo, "according to Prieſtley's experiments, by weak electri- "city poſſeſſing great velocity*."—Here then the fluid paſſes 'with difficulty through water,' *ye conjurers!*—— But why do I argue againſt ſuch abſurdities? And yet

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"EXPER. 2. If zinc, iron, or copper, are employed with gold, in dilute nitric acid, nitrous gas is formed, in the ſame manner, and under the ſame circumſtances, as the hydrogen gas in the former experiment.

"EXPER. 3. Experiments analagous to the former, and equally ſimple, may alſo be made with many metallic ſolutions. If, for inſtance, the ſolution contains copper, it will be precipitated by a piece of iron, and appear on its ſurface. Upon ſilver merely immerſed in the ſame ſolution, no ſuch effect is produced; but as ſoon as the two metals are brought into contact, the ſilver receives a coating of copper."——*Doctor Wollaſton's Experiments on the chemical Production and Agency of Electricity;* PHIL. MAG. vol. xi. page 206.

[*] PHILOSOPHICAL MAGAZINE, vol. xi. page 272.

I *muſt*, in order to open the eyes of the *ſervum pecus*, which they have thoroughly cloſed.

Allow me juſt to recapitulate—that the electrical fluid, ſet looſe by the ſurface of the zinc, penetrates through the zinc, being attracted by the ſilver which now touches it and paſſes through it, and then through the water to the zinc again*. Then what are the phenomena from this circulation? Why, that this fluid, when it paſſes through the ſilver, attracts ſo much phlogiſton, or fixed fire, that when it penetrates the water it receives its water of compoſition, and then forms inflammable air†.—How beautiful, and yet how ſimple is this illuſtration of the phenomena! Let us now ſee what the French theory ſays—

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* Mr. Davy is forced to ſay, "But little knowledge has yet been obtained concerning the chemical changes taking place in the ſecond claſs of Galvanic batteries. But from ſeveral experiments it would appear that they are materially different in the laws of their production from thoſe taking place in the firſt claſs.

"Thus, when ſingle metallic wires, with water, are placed as ſeries powerful batteries of the ſecond order, the influence producing oxygen ſeems to be tranſmitted by the point, in the place of that part of the plate, which was apparently incapable of undergoing oxidation; whilſt the hydrogen is evolved from that point where the oxidating part of the primary ſeries appeared to exiſt."—PHIL. MAG. vol. xi. page 336.

Now what muſt the candid reader think? Muſt I argue againſt the abſurd idea of the ſecond glaſs of batteries being different?—Our infatuated French theoriſts can, with all the *nonchalance* imaginable, ſay black is white; and this they will call philoſophy. It may be philoſophy at the Inſtitute, but in every other place it is groſs nonſenſe.

† Mr. Nicholſon's Journal for June 1804, p. 85, ſays, "I was lately rather ſurprized to find that Galvaniſm had the effect of making ſilver remarkably brittle. This looks as if its action was ſomehow connected with the malleability of the perfect metals, and to ſhew that the conducting metals ſhould be occaſionally paſſed through the fire. The ſilver plates were nearly clean."—*Letter to Mr. Nicholſon.*

The water is decompounded at the zinc: but then they ſay that the electrical fluid ſeizes upon the hydrogen let looſe by the oxygen, and flies with it to the ſilver, and then moſt unpolitely gives it a kick, making it fly off as hydrogen gas. Now it is certainly moſt unpolite, thus to ſever it from its dear paramour *(Oxygen)*, they having ſuch a violent *attachment* to each other: therefore the *attraction* for the electrical fluid muſt be ſtrong indeed, to overcome the other; and yet in travelling with it but a ſhort way to the ſilver, it turns it off as a vagrant.—— But ſtill we are more embarraſſed; for it appears that hydrogen is formed at both metals—that there is an evident calcination at the zinc, but not at the ſilver. How comes this, *according to their theory?* Here they muſt be mute. But let us examine it, *according to mine.* The watery acid calcines the zinc, and ſets looſe its fixed fire as hydrogen gas; the electric fluid in conſequence is ſet looſe, which then is attracted by the uncalcined zinc, now becoming *plus;* and as ſoon as the ſilver touches it, it will impart to the ſilver a portion of its electricity,— which then circulates through it to the water; and in the progreſs of this circulation, it attracts a quantity of the fixed fire of the metals; ſo that, upon reaching the water, they form, *together*, hydrogen gas.

Now let us conſider the ſecond experiment.— In this experiment the nitrous air is only formed, and no hydro-

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Now I ſhould ſuppoſe that even the French votariſts would admit that this is owing to taking its phlogiſton from it. But in uſing iron wires for a long time, I have, *in a manner*, made the calx thro' all their ſubſtance; and if the gentleman had conſulted my writings, he would have been under no ſurpriſe.

gen gas. The *Diable!* This is a huger mountain to remove, a more inſuperable difficulty than the former.—What will the French theoriſts ſay to this? The electrical fluid muſt ſeize upon the oxygen of the nitrous acid at the zinc metal, (leaving the poor water undiſturbed); and then flying with it to the ſilver, it becomes ſo enamoured of it, that it will not let it eſcape, as it did the hydrogen. Alſo, it actually ſeizes upon the oxygen of the nitrous acid that was contiguous to the ſilver wire, for nitrous air is alſo formed there. But then the matter of aſtoniſhment is, what comes of all the oxygen, as the gold is not oxidated? The proceſs ſtill goes on, forming nitrous air, and ſtill no oxygen appears! *Is it annihilated?* Where the *Diable* is it gone? for there is neither key-hole nor barrel for it to leap through. This is a feat far more extraordinary than the bottle-conjurer in the Hay-market. *Servum pecus*, can you *digeſt* this, as they have made you *ſwallow* it? There is a chance of your ſtomachs bringing your heads to their ſenſes, at laſt. I think, my countrymen, you will have enough of French philoſophy and philoſophers, in the end.

Let the ſame experiment be next examined by my theory.—The nitrous acid and water attack the zinc, ſeize upon its phlogiſton, and form nitrous air. The electrical fluid, in paſſing through the metals, attracts ſo much of their fixed fire, as that when it paſſes out of the ſilver it is highly phlogiſticated and attracts the nitrous acid, and alſo forms nitrous air; with the vitriolic acid it forms ſulphur.

The third experiment juſt bears the ſame rational explanation with my theory, but with their's ariſes the ſame abſurdity.

Dr. Wollaſton broaches a ridiculous theory, that all electricity is owing to the calcination of metals. If the Doctor will take his cat, and rub her back well with his hand, electricity will be excited: but in this caſe, *pray where are the metals?* In ſhort, the Moon is a great Cheſhire Cheeſe, merely becauſe it is the ſhape of one; *full of maggots*, good Doctor!——*O *cunning rogue!*

The electrical fluid is formed of an acid, (I ſuppoſe the nitrous,) and fire. This active fluid, circulating through the pile, and flowing ſtrongly upon the oxidable metal of the pile, oxidates it. As the flame of a candle, upon the combuſtible body it is blown upon, burns it; while the body which it is blown from, is not burnt by it. And that this is the caſe with the electrical fluid, ſtrikingly appears from an experiment of Mr. Cuthbertſon's in Nicholſon's Journal.

Moreover, though the electrical fluid contains fire, and an acid capable of calcining metallic bodies of itſelf, yet, in paſſing through the pile in the diſks, it meets with water, acids, alkalies, neutral ſalts, and atmoſpheric air; all of which conſiderably aid the calcination:—ſo that when the electrical fluid is highly ſaturated with the phlogiſton (or fixed fire) which it acquires from the metals of the pile, an actual combuſtion takes place; its own fixed fire which it received from the metals, and alſo the phlogiſton of the metal of the zinc ſide, are ſet looſe as free fire. That is, when plates are large, ſo as to be much expoſed to the atmoſphere, and the bodies which it finds in the diſks, (that is, betwixt the plates); and which combuſtion, producing ſuch an active ſtate of fire ſo as actually to inflame the metals oppoſed to its circuit,—will produce very little effect in forming airs, or in giving the electrical ſhock to animals. Indeed, inſtead of adding to theſe effects, I have found the electrical fire, in a great meaſure, decompounded,—its fire becoming actual, or ſet looſe, from its acid which was depoſited in the proceſs. And, on the other hand, where the nitrous ammonia was placed in the diſks of a pile with leſs plates, I have increaſed the electrical fluid. What induced me at firſt to form theſe opinions, I may probably detail hereafter.

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* This epithet, as well as the annexed ſubſtantive, is merely in conformity with the North-of-England dialect, to children, when they ſhew any remarkable archneſs.

There is a difference, I have found, in the electrical fluid of the common machine and the pile. The former is formed of Nature's ſaturation of fire, and the acid;—but in the pile the acid gets a higher ſaturation of fixed fire from the bodies which compoſe the pile, and which in conſequence makes it better adapted to form airs:—alſo it cauſes its operations upon the human body to be more active, and is better calculated to remove diſeaſe. The circulation of the fluid in the pile is likewiſe conſiderably ſlower than in the common machine. The *plus* and *minus* are conſiderably higher in the common machine, being formed by the Leyden phial while in the pile, owing to the little obſtruction that is given to it by its paſſing through the fluids in the diſk. Alſo the Galvanic fire is more fixed than in the common machine, and therefore leſs repulſive—and which repulſion is the cauſe of its active motion, as I have ſhewn. Indeed the electrical fire of the pile differs very much; for in the large plates its fire becomes more actual by combuſtion than in the common machine, as may be ſeen in its ſetting fire to the metals. A more ſull inveſtigation of all theſe phenomena, I may probably give hereafter.

There are two intereſting experiments, which I was extremely deſirous ſhould be tried by other philoſophers; and a long time ago, I wrote to the gentlemen of the Aſkeſian Society, informing them, that if the propoſal met with their approbation, I would ſend up the experiments to the Society, in order to have them repeated; but hitherto I have received no anſwer to my letter*.——The firſt of theſe experiments conſiſted in forming a Torricellian Vacuum, with quickſilver, in a long glaſs tube, having a little round head at the top. Through this part of the glaſs were formed two holes, in which were accurately inſerted a piece of braſs in each, ſo as to admit of two ſmall wires, pointed at the end, with a mixture of ſulphur and iron. Upon applying theſe two wires to an active Galvanic battery, the mixture of ſulphur and iron were ſtrongly ignited, and, after the ignition, they became vitriolated iron. Here then was no oxygen in the proceſs.—⁂ This experiment anſwers equally well, if you melt about one part of ſulphur with three of iron, and form it into the ſhape of a piſtol ball, and when hot, run into it two golden wires:- then apply theſe wires to the machine,—which having worked for

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[*] A gentleman connected with this Society, was ſo obliging as to aſſure me that he would mention the circumſtance to the Preſident of the ſaid Society, who politely informed him, that he had not ſeen the letter, but that the Aſkeſian Society would always be happy in receiving communications from me. But as I then was determined to ſend abroad theſe experiments through another channel, I did not ſend them up;—and now they are publiſhed, I ſhall be much obliged to the Preſident if he will get them repeated, and do me the honour of acquainting me with the reſult.

ſome time, *ſplit the ball*, and you will find that part of it (directly in the centre, betwixt the wires which had the electrical fluid circulating) will become the vitriolated iron. Now this is only done in the innermoſt part of the ball, which is inacceſſible to the air.

The other experiment conſiſted in collecting as large a quantity of the air that is formed in the Galvanic pile, (which they call oxygen), and to try if it was reſpirable, by placing a common *bee* in it, which died immediately, the ſame as it would have done in any noxious air.—— It is reaſonable to ſuppoſe, that in London, where they have the advantage of machines on an extended ſcale, they might collect as much of this air as to place *a mouſe* in it.

That both what they call the inflammable air, and oxygen gas, are of the nature of the gaſeous oxyde of azote, appears from the examination of theſe airs, upon firing them. Mr. Cruickſhanks ſays, that "where the "gaſes obtained by gold and platina wires, are collected "together and exploded over mercury, the whole nearly "diſappears, and form water, with probably a little nitrous acid, for there was always a thick nitrous vapour "perceived for ſome time after the exploſion. The reſiduary gas in this caſe appeared to be azote*."

Dr. Prieſtley (in his 6th volume, page 335) ſays, "But "the moſt effectual change that I was able to make in "this kind of air, *(dephlogiſticated nitrous air)* was bringing it to a ſtate in which I doubt not it was reſpirable, "by means of the electric ſpark. Taking this ſpark, or "making ſmall exploſions, in 0.4 of an ounce meaſure of

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* Nicholſon's Journal for September, 1800—page 257.

"dephlogiſticated nitrous air, it was rendered wholly "immiſcible with water, and of the ſtandard of 1.45."

Dr. Prieſtley alſo, (in his 2d volume, page 128) ſays, "A quantity of this air, *(the gaſeous oxyde of azote,)* received in water, was about half-abſorbed in one night. "By agitation it appeared to be abſorbed not ſo readily "as fixed air, nor with ſo much difficulty as nitrous air, "but in a medium between both. When this air was "reduced to about one-eighth of its original bulk, it was "diminiſhed by nitrous air. But this is the caſe with all "the kinds of air that will bear the experiment, and even "with nitrous air itſelf, as I have obſerved in my former "publication."

Mr. Davy, this is certainly the wonderful *Laputatic air* that made you dance ſo!

Dr. Wollaſton affirms, that the poſitive wire in the common electrical machine turned an infuſion of litmus *red*, and the negative wire reſtored it to its original *blue* colour. Now this only informs us that there is an acid in the electrical fluid, which vegetable infuſions have a ſtrong attraction for, and which ſeizes upon it; but that when this red infuſion is expoſed to the negative wire, its newly-acquired acid is again attracted by the phlogiſton which the electrical fluid had attracted from the wires, and bereaves the vegetable juices of it. This fact, Mr. Cavendiſh, is ſufficient to teach you how the cauſtic alkali received its acid in your experiments, and that it was not from the atmoſpheric air being decompounded; for it is now known that the electrical fire poſſeſſes an acid. As to the common electrical machine forming air, as well as the Galvanic pile, theſe phenomena I ſhewed long before Dr. Wollaſton's publication was uſhered into the world.

I hope my reader is now fully ſenſible that ammonia is formed of phlogiſton, and that the phlogiſton of metals forms it: this (I think) I have ſatisfactorily proved; yet, to corroborate the poſition, I beg leave to ſtate,—that if the Galvanic proceſs be prolonged, (*i. e.* ſlowly continued,) and if the wires are long and thick, the hydrogen (of itſelf) will not have heat enough to form all into hydrogen, but a part of it will be depoſited in the water, which will be found to hold in ſolution an alkali. So that Mr. Davy ſays, page 336, "If the proceſs be con-" tinued for ſome time, the apparatus being expoſed to "the atmoſphere"— *(but I have found the atmoſphere of no conſequence in the proceſs)*—"the water in the oxygen-" giving tube, will become impregnated with an acid, "(apparently the nitrous); whilſt that in the hydrogen-" giving tube, will be found to hold in ſolution an alkali, "which in certain caſes has appeared to be fixed."—— Now I am deceived, if theſe circumſtances do not ſtrongly indicate, that the voluble and fixed alkalies are nearly allied to each other, and both formed of fixed fire, which I have all along inculcated.

In examining Nicholſon's Journal for July, 1804, I ſee that Mr. Ritter, a very ingenious and able experimenter, has made ſome intereſting experiments on the Galvanic pile, which I ſhall examine. He ſays, (page 176,) "But it was never yet ſuſpected, that the whole "pile becomes negatively electrified, when a communi-" cation is made betwixt the poſitive pole and the earth, "by means of ſome conducting ſubſtance; and, on the "contrary, the whole pile becomes poſitive, when the "electricity is abſtracted from the negative pole. We "have here a phenomenon, that ſhews the theory of

" electricity to be ſtill in its infancy. When the ſtate " of the pile is thus changed, its chemical action is not " deſtroyed, but continues as before."

Now let us ſee how this fact agrees with *my* theory. When a communication is made between the poſitive pole and the earth, (which is, *the zinc pole)*, from its nature its electricity will diffuſe itſelf into the earth, and the pile will become negatively electrified. But when the communication is made between the negative pole, (which is, *the ſilver pole)*, and the earth—as its electricity is received by the earth, the earth will receive a great deal of its electricity, being repelled into it;—but not the whole: ſo that the whole pile will become poſitive. For in this caſe, the electrical tenſion between the two poles is taken off, in conſequence. What electricity the pile poſſeſſes, is ſtill diffuſed through the whole of it, uniformly.

In the firſt inſtance, the earth, by communicating with that part of the pile, namely, *the poſitive pole*, will receive an amazing quantity of the fluid, ſo as to make what remains of it negative; but when the earth communicates with *the negative pole*, the quantity it receives from it is only by repulſion; that is, the electrical fluid is repelled into it by the tenſion which it experiences in the pile, from *the poſitive pole* to *the negative pole.*

It is ſaid, " We have here a phenomenon, that ſhews " the theory of electricity to be ſtill in its infancy."—— Thus, according to *their* theories, it is inexplicable, and ſtands in direct oppoſition to all former facts——but, viewed in the light of *my* theory, it is rational, and harmonizes with former facts. The pile being thus changed, its action goes on as before; for when the communication is made by good conductors between the poles, the circulation of the fluid that is left, and ſtill generating, will take that courſe, inſtead of going into the earth.—But its action will be greatly leſſened; for Dr. Marum found it was neceſſary to have the action of the pile in its full vigour, to inſulate it. Alſo, after theſe changes take place, by its poles communicating with the earth, to ſhew that the pile has loſt a great quantity of its fluid, is, that if it ſhould be tried juſt previous to this communication with the earth, and afterwards, it will be found conſiderably weaker.

The next experiment which he makes, is to ſhew, (what we might have ſuppoſed *à priori,)* that the pile charges a jar accordingly as its fluid moves rapidly or ſlowly within itſelf. For why it charges a battery ſo inſtantaneouſly, is owing to its fluid moving rapidly in a circle within itſelf: therefore a battery being expoſed to its circuit, will naturally receive its fluid, and accordingly as the fluid moves quickly or ſlowly in it, in like manner it will be received by a quick or ſlow progreſſion.

"*It is a well-known fact, that electricity produces the* "*ſame change in water as Galvaniſm.*" This fact I proved before either Dr. Van Marum or Dr. Wollaſton. "Ritter has ſhewn that poſitive electricity, like poſitive Galvaniſm, diſengages from it oxygen gas; and that negative electricity, like negative Galvaniſm, diſengages from it hydrogen."

The electrical fluid is formed of fire and an acid, as I have ſhewn in my former works. If this fluid is received from a jar, its ſaturation of fire principally form what they call oxygen gas; but when the ſame fluid circulates through metallic bodies, it will attract a higher ſaturation of fixed fire, and principally form with water what they call hydrogen gas. And according to the metal which it circulates through, is eaſy or difficult to calcine; that is, to part with its fixed fire, the electric fluid will be ſaturated with it accordingly. Or, in ſuch proportion as the metal is long and ſmall, it will be expoſed to the fluid, and be robbed by it of its phlogiſton accordingly. All this I fully proved.

He alſo found, that "the electricity does not produce "oxygenations and hydrogenations in the humid way "alone, but in the dry way alſo." Then, *clearly*, this ſhews, if there is allowed to be any common ſenſe and reaſon in theſe diſquiſitions, that water does not furniſh the oxygen and hydrogen found in theſe proceſſes.== "In a pile (ſays Mr. Ritter) the poles of which are not "made to communicate by means of a conducting ſub"ſtance, the chemical action of the ſtrata compoſing it "is very unequal. The plates of zinc are oxided leſs in "proportion to their diſtance from the poſitive pole; ſo "that thoſe neareſt the negative pole have frequently no "traces of oxidation, and ſeem rather to have been pro"tected from the action of the water by which they are "wetted, than attacked by the action of the pile."== (*Nicholſon's Journal for July*, 1804—page 178.)

The above ſufficiently demonſtrates, that it is the circulation of the electric fluid that produces the calcination; for when it is retarded, the oxidation is retarded alſo; and when, like a river, it is obſtructed, as it overcomes that obſtruction, it will move quicker, till it circulates to the obſtruction again, and, as it approaches the obſtruction, it will gradually decline in its motion, proceeding ſlower and ſlower.—Thus it is in the proceſs of calcination.

To aid the action of the pile upon the animal body, Mr. Ritter ſhews how neceſſary it is that the ſkin ſhould be wetted, to add to its conducting power; and to make its action fall more upon a particular part, the conductor which is applied to *the part*, muſt be ſmall, and that this makes its action upon it the ſtronger. This is preciſely the ſame train of reaſoning which I have employed, in deſcribing its action upon metals; the ſmaller, the ſtronger action; and the longer and the ſmaller the wires are, airs are accordingly produced.

Mr. Nicholſon, in his *marginal labours*, ſays, "Ritter "reduces all the effect of the pile on the body to expanſion and contraction; the poſitive conductor expanding, "the negative contracting the part. The pulſe is made "ſtronger by the poſitive conductor; weaker by the negative. The expanſion is followed by a ſenſation of "heat, and *vice verſa*. The action of the pile on the organs of ſenſe depends on the nature of the organ; but "the two poles produce the extremes of the ſenſation.— "The eye, in the poſitive ſtate, ſees objects red, large, "and diſtinct; in the negative, blue, ſmall, and more "obſcure. On the tongue, the poſitive pole produces "an acid taſte; the negative, an alkaline. In the noſe, "the negative produces an alkaline ſmell; the poſitive, "that of oxygenated muriatic acid. In the ears, the poſitive produces a grave ſound; the negative, an acute." *Page* 180.

Upon *my* theory, we are not at a loſs for an eaſy and rational explanation of theſe phenomena—"the poſitive "conductor expanding, the negative contracting the "part." *My* theory reverſes theſe poles. It ſuppoſes the ſilver wire the poſitive, and the zinc the negative:— then the fluid flows from the ſilver wire to the zinc wire:

the fluid no ſooner begins to enter the body than it meets with much impediment in paſſing the ſkin, which requires to be moiſtened; when, overcoming *that*, it circulates freely till it comes to the ſkin again, to paſs out of the body to the zinc wire; but meeting with this impediment, it accumulates until it has overcome it again; juſt the ſame as a river. In conſequence of which, (the nature of the fluid poſſeſſing ſo much fire) it will expand the part; and ſo *vice verſa*. The part it enters into will direct its fluids from it to the part it paſſes out of.══ But, according to *their* opinion of its circulation, it ought to have been reverſed. The pulſe is acted upon juſt in the ſame manner: its *ſtimulus* is the greateſt, where it is moſt accumulated; — and the circumſtance of its being made weaker by the ſilver wire, is probably partly owing to its duſturbing in its courſe, and diverting from its natural channel the nervous fluid, taking it from the arm it enters to the arm it paſſes out at. The eye and ear are affected in the ſame way. The *retina* being more ſenſible, by reaſon of the accumulation of both the fluids, (namely, *electric* and *nervous,)* before the former penetrates out of the body to the zinc wire, the eye will perceive objects *red*, *large* and *diſtinct;* and in the negative, *blue*, *ſmall*, and more *obſcure*. This fully elucidates my Theory of VISION, which I publiſhed in 1796, that colours are owing to the degree of *ſtimulus* which the *retina* receives by the ray of light, according to the force with which it is repelled againſt the *retina*, (or nerve of the eye); and which has been attempted to be pilfered by others. It is this Theory which confounds Sir Iſaac Newton's hypotheſis, that *a ray of light conſiſts of different colours.*—It is plain, that the fact above ſtated, while it confirms my Theory, it renders that of Sir Iſaac Newton erroneous*.

The tongue is affected with an acid taſte by the zinc pole, and an alkaline taſte by the ſilver pole. As the acid circulates, the electric fluid is highly phlogiſticated, *as ammonia;* the other is more in its natural acid ſtate: ſo the noſe finds in the ſilver wire an alkaline ſmell, and in the zinc wire an acid one. The ears are equally affected with the eyes. Nothing can ſpeak more ſtrongly in favour of my ſyſtem than theſe facts, nor more flatly contradict their's.

Mr. Cruickſhanks proves that an acid (viz. *the nitrous)* is depoſited in the diſks. Alſo Mr. Davy ſays, that both 'an acid and alkali' are depoſited by the different wires. Likewiſe Dr. Wollaſton ſhews, that vegetable juices are turned red and green by the different wires. Now as all their own experimenters bring forward theſe different facts, can we be ſurpriſed that this acid, ſaturating itſelf with the phlogiſton of the metals, ſhould produce different airs? For, if there exiſts a fact in chemiſtry, I have proved that the nitrous acid and phlogiſton produce both inflammable airs, and alſo what they call *the gaſeous oxyde*

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* That obſcureſt of all writers, Dr. Young, upon ſeeing how inſufficient I have ſhewn Sir Iſaac Newton's doctrine in accounting for the phenomena of colours, and how juſtly mine explains them,—has endeavoured to drag into the explanation the action of that *imaginary* body, ÆTHER, producing undulations. I can only ſay, (for he deſerves no other anſwer) that his doctrine is as *abſurd*, *obſcure*, and DARK as a candle placed in a crooked cylindrical veſſel, ſo that not a ray of light from the candle can paſs through it; yet admits freely of the paſſage of the ſound of the learned Doctor's obſcurities and abſurdities, in defence of his opinion,—proceeding from his head with all the pompoſity of Friar Bacon's Brazen-head, when unfolding future events.

of azote; and that theſe airs may be meliorated by agitation in water, which they muſt experience in the diſks, as Dr. Prieſtley's experiments, juſt quoted, ſufficiently prove.—Now, the airs produced by the pile, are *airs directly ſimilar*, I poſitively aver, and call upon the public to examine them. Indeed their own experiments prove it: for, upon firing them, Mr. Cruickſhanks ſays, that "when the gaſſes obtained by gold or platina wires, are collected together and exploded over mercury, the whole nearly diſappears, and forms water,—with probably a little nitrous acid, for there was always a thick nitrous vapour perceived for ſome time after the exploſion." But I have examined them by various proceſſes, the reſult of which has conſtantly been, that *they are identically theſe kinds of airs*, and that there is not an atom of the pure air of the atmoſpere in them, (what they call oxygen gas). This I directly and unequivocally aſſert, and dare them to ſhew the contrary*.

Another deſcription of philoſophers has of late ſprung up, who, ſeeing the abſurdity of the doctrine of water being formed of oxygen and hydrogen, have formed a new theory, and ſay that poſitive electricity and water form oxygen gas, and negative electricity and water, hydrogen gas.—I can ſcarely ſubmit to the drudgery either of ſtating or refuting ſuch abſurdities, which carry contradiction on the face of them. Theſe gentlemen have never attended to the different phenomena which have been ſhewn by the experiments of theſe laſt thirty years, but ground their aſſertion immediately upon the ſingle fact of *theſe airs being produced by the Galvanic pile.* With as much colour of reaſon I may aſſert, that the Moon is a large Cheſhire Cheeſe, from this fact, that *it reſembles one in appearance.* Oxygen gas is produced in the greateſt abundance, by fire, from nitre: Dr. Prieſtley produced about half its weight into oxygen gas and azote gas.—Ammonia may be all formed into hydrogen gas, by fire. Now, in the firſt inſtance, the fire muſt have been decompounded in paſſing into the nitre; its *minus* electricity muſt have gone to the formation of the oxygen gas. Then what an accumulation of the *plus* electricity there muſt have been in the reſiduum! (viz. the cauſtic alkali.) Alſo, in forming the hydrogen from the cauſtic ammonia, what became of the *minus* electricity? Beſides, from whences comes the water neceſſary to theſe airs? Mr. Kirwan ſays, that 960 grains of nitre contain about 326 of the nitrous acid, and 115 of water. Hence it appears, that Doctor Prieſtley produced more air from nitre, than the weight of both the acid and water put together:—from 960 grains of nitre, he produced 487 grains of air.—See the 6th volume, page 294.

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* Mr. Davy has an experiment which I have not noticed, *upon azote being abſorbed, by reſpiration, into the blood.* He endeavours to ſhew, I am told, that "a portion of azote diſappears in reſpiration, and another portion is extricated during the proceſs."—Good Heavens! is it poſſible to aſcertain ſuch a thing by experiment? To demonſtrate the fact, a man muſt be reſpired by the lungs, and actually accompany the azote into the blood, and afterwards take his departure with the other portion of azote,—ſeparate from the abſurdity which there is in ſuppoſing that Nature's great office in that important function is to abſorb and evacuate the ſelf-ſame body by the ſelf-ſame operation! But ſuch are their experiments, and ſuch their opinions. Philoſophy is now conducted as a Fairy Tale: there is hypotheſis upon hypotheſis, abſurdity upon abſurdity, *Laputiſm* upon *Laputiſm*. Every thing is *aerial*—there is not an atom of *ſolidity*, any common ſenſe.

The cauſtic volatile alkali poſſeſſes no water, they ſay. But it ought to have been all water, as it is all turned to hydrogen, they allow.—Mr. Cruickſhanks, by paſſing the Galvanic fluid through ammonia, formed it into inflammable air. This, then, muſt have been accompliſhed by the *minus* electricity—only the following difficulties occur: In this caſe, what became of the *plus* electricity? How came the machine to work, when one electricity was received from it, and not the other?—— But, in ſhort, if the reader cannot hunt down theſe puerilities himſelf, his head is not calculated for philoſophy. *I am tired;* for my toils have not been inferior to the celebrated labours of Herculus, in cleanſing the Augean ſtable.

But, more particularly, what comes of all the nitrous acid in the nitre? and from whence comes the azote?—Beſides, if we are to carry with our diſcuſſion common ſenſe, (and not be altogether *Laputatic*, launching into *fairy* regions, where Fancy governs and Reaſon is trodden under-foot,)—the electric fire (or fluid) contains a great quantity of actual or free fire, ſeen in its inflaming gunpowder and melting iron:—therefore it would undoubtedly form bodies into gaſſes; and alſo it is well known that it will reduce the oxydes of metals.—It is equally well known, too, that where bodies are electrified *plus*, and bodies are electrified *minus*, if there is a communication made between them by an iron wire, there will be found likewiſe *a communication of electricity*, attended with the electric fluid becoming viſible. Nitre, and combuſtible bodies, as ammonia, (which are nevertheleſs *ſolid* bodies,) will burn with great brilliancy.—Then how is this fire conducted, upon the principles of this *ſublime* hypotheſis? The oxygen in the nitre muſt attract the hydrogen of the ammonia, and their *minus* and *plus* electricity muſt form the combuſtion. Heavens! what a quantity of electricity they muſt contain! for the combuſtion may be carried on for hours, as nitre and charcoal. Does charcoal, then, contain the *minus* electricity, as well as hydrogen? *What an aſtoniſhing diſcovery!* But then the product in firing the one is *water*, and the other *fixed air*, they ſay. Is there any reſemblance between theſe two bodies, *ye wiſeacres!* This acid air then muſt be formed of charcoal and water, according to the theory we have been examining. Theſe are the elements of *Laputiſm*; or rather, to uſe a *bright* ſimily of the Reviewers, the whole is "*a vaſt cloud of ſmoke*," encompaſſing the Genius of Modern Philoſophy: *See page* 233.

But if you take an iron wire, and make a communication between them, there is no electricity communicated; or place either the nitre or ammonia in an electrometer, they will ſhew no ſigns of electricity. Good Heavens!—But this age is full of revolutions, and ſcience has fallen in for its ſhare. Common ſenſe has been kicked out of doors by the revolutionary principles of French philoſophy, which have introduced in its ſtead the *Laputatic* extravagancies of modern nonſenſe. All the *canonized* principles of our venerable anceſtors are rejected. Theſe reſtleſs innovators have therefore no fixed principles to reſort to; there is neither *rein* nor *curb* to govern their wild imaginations; hence the introduction of every thing that is frivolous, abſurd, and ridiculous! In a word, THIS IS A BALLOON AGE. Swift's imaginary iſland is at length eclipſed by bolder fictions. Nor can I whip theſe men into ſolid ſenſe; the machinations of this ever-to-be-deteſted confederacy counteract me. But the day of retribution will ſoon come, I hope, for the reception of my truths. The celebrated Mr. De Luc has opened *his* eyes, and has begun (as our Reviewers ſay,) to attack "WITH FEROCITY" the French Theory of Chemiſtry, charging it with being an enemy to the progreſs of ſcience, and ſhewing *(after me,)* that it makes no diſtinctions between vapours and permanent gaſſes.

In fine, carry this late theory into the elaboratory, and examine all the different chemical facts by it, for I have only done ſo in the two firſt caſes that preſented themſelves. *Servum pecus!* need I argue any further? . . . . It is greatly to be lamented that our philoſophers have no fixed *data* to proceed upon. Their brains have become ſo deplorably addled, and their faculties perverted, by poring ſo long over the *aerial* flights of French philoſophy, that they are ready to adopt and ſwallow any thing, however prepoſterous and abſurd. For the edification of ſuch, I will venture to tell them a ſtory, *accounting for the origin of the Moon.*

In a far remote period of time, a Califf, who ruled over the extenſive country of Arabia, was particularly fond of Cheeſe; in truth he was an epicure, yet ſagacious enough to know, that the larger the body of Cheeſe made together, the richer it would be; he therefore commanded his ſubjects to bring all the curd from the milk of their reſpective dairies, which they were directed to place upon a lofty mountain, whoſe ſummit he had prepared ready to receive the precious depoſit; and that nothing might be a-wanting to carry his reſolution into effect, he had contrived the moſt ſtupendous machinery to convert this immenſe maſs of curd into Cheeſe. Beneath this enormous load, the produce of myriads of animals, Nature groaned; and, as if diſdaining the burden, by a ſudden concuſſion of the earth, (which ſometimes happens,) ſhe whirled high into the upper regions, this huge and unwieldy Cheeſe, the diameter of which was ſuppoſed to be ſome hundreds of furlongs! From this impulſe, (according to our *wiſe philoſophers,)* and gravitation together, this famous Cheeſe has ever ſince revolved regularly round the earth. But as all things are ſubject to decay, ſo this Cheeſe alſo, in proceſs of time, ſhewed ſigns of *corruption,* the forerunner of *diſſolution.* In plain Engliſh, it engendered MAGGOTS, which occaſionally drop down upon the HEADS of our philoſophers, garniſhing them with *new* ideas, and influencing their *diſcoveries* even to aſtoniſhment! Nor need we be ſurpriſed at this, ſince the dark ſpots in the ſun (as Dr. Herſchell has ſhewn) influence the price of grain in Mark-lane!—What a wonderful age is this we live in!

But that I do not make aſſertions without the authority of proof, I ſhall juſt quote TWO inſtances of the wonder-working effects of theſe MAGGOTS, which are very troubleſome little animals, *where they bite!*——The firſt *ſubject* of their wanton caprice, that I ſhall mention, is a Mr. Wilkinſon, of whom the Reviewers report that he ſays, (for indeed I have not ſeen the production)——

'He conſiders it as an important Galvanic operation, and
'the lungs themſelves as affording a well-marked inſtance
'of a ſtructure ſimilar to that of the electric organs of the
'*torpedo:* by the conſtant ſucceſſion of ſhocks, (which is
'ſuppoſed to be communicated from the lungs,) a *ſtimu-*
'*lus* is afforded to the "organized and animated parts of
'the body, by which heat is produced."

So the Moon is a large Electrical Machine, which has conductors to the wiſe and learned heads of our philoſophers, and produces, by its ſhocks, theſe wonderful diſcoveries!

* But, reader, in compaſſion to thy feelings, I forbear to exerciſe thy patience any further;—I will no longer expatiate on the ridiculouſneſs and groſs abſurdity of our pſeudo-philoſophers, whoſe productions rank lower than the *Hiſtory of Tom Thumb*, or *Jack the Giant-Killer;* and their philoſophy no one can endure—even the Nurſery would ridicule it.—This is the effect of the *maggot* with a vengeance!

But, let us conſider their prototype—then we need not wonder at their *imitators*. When they account (or *pretend* to account) for *chemical phenomina,* it is diverting to ſee how they *prance* away at a high *Gallic* movement†.—

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* "Let us make one other ſtep, in a few words. The powers of the Galvanic pile are limited by the oxydation of the diſks. The animal machine is comparatively uncharged, and weakened only by exertions. We know that Galvaniſm decompoſes the water, and oxydates the metals.—In water the azote predominates; and animal fluids are azotic: the moſt excrementitious ones are azotic in a great degree, or, as they are ſtyled, highly animaliſed. Thus, in the animal proceſs, by the conſtant power of Galvaniſm acting in producing the muſcular motions, the watery fluid is expended, and we require a ſupply; the azote combines with the fluids, and is thrown off; the oxygen gives out its caloric, and ſupplies the body with heat."——*Appendix to the 39th Volume of the Critical Review,* page 535.—(Sue's Hiſtory of Galvaniſm.)

Now this Critic's critical brain muſt be a weak watery one, upon his own principles; and this may account for the watery opinions of our modern philoſophers.

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† The Appendix to the Critical Review for September, 1804, which I have now in my hand, treating upon the French Chemical Annals, in accounting for the Pruſſic acid, has the following paſſage:——

Reader, canſt thou digeſt ſuch abſurdities? This is altogether in the ſtyle of an Eaſtern Tale of *Genii*, *Enchantments*, *Palaces*, and *Magicians*. Their *radicals* are Oxygen, Azote, Hydrogen and Carbon! But *I* ſay, their *radicals* are Ignorance—Folly—Vanity—and Abſurdity! And let theſe *radicals* run the ſame gauntlet, and they will produce--not the Pruſſian acid, but the Gallic theory of chemiſtry! It makes me ſmile, when I anticipate the opinion of future ages upon this frivolous and revolutionary age. What will they call it? The *Laputatic*, *Lunatic*, or *Aerial Caſtle-Building Age?*——O ye SOLOMONs! ye will certainly be immortalized[*]!

“ *Memoirs on the Nature and new Properties of the Pruſſic acid. By* M. Curaudau.”

“ It appears from our author, that the Pruſſic calcination contains only two principles of the radical, viz. azote and carbon. The third is afterwards added; and in this firſt proceſs our author ſeems to have diſcovered a new agent, which he calls carbonated azote of potaſh. This ſubſtance immediately decompoſes water, and thus favours the oxygenation of the carbon, and the union of the hydrogen with the azote and carbon. The union of this radical with different ſubſtances will of courſe obtain different names. The hydrogen is not originally combined in a ſufficient proportion to form ammonia, but acquires it by a further action and decompoſition of water. A part of the radical may be decompoſed, by conſtantly moiſtening the Pruſſic calcination with a ſolution of ſulfate of iron at its maximum of oxygenation; and the radical only becomes an acid from the union of the oxygen, furniſhed by ſome metallic air neceſſary to its formation.”——*Page* 521.

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[*] I appeal to my countrymen, what do the Engliſh men deſerve who endeavour by the moſt vile arts to ſuppreſs my diſcoveries, and exalt the French theories, when “we ſee the moſt prejudiced and jealous conduct of the French in favour of their own nation?” The Monthly Review, whoſe conduct towards me has been ſhameful in the extreme, exalting the French philoſophy, ſays, “ Our Gallic neighbours are ſo cloſely wrapped

In all caſes where the calces of metals are ſaid to be highly impregnated with oxygen, it is done by calcination, or imbibing fire. Thus manganeſe, if carbonated, is white; but by calcination it becomes black: (ſee page 22:) lead the ſame. The white calx of lead becomes red by calcination: the calx of iron the ſame,—turning, by high calcination, to colcothar. Mr. Smithſon has obſerved, that all calces change their colour by fire. He ſays, "This quality of temporarily changing their colour by heat, is common to moſt, if not all, metallic oxydes; the white growing yellow, the yellow red, the red black." But fortunately I have juſt ſtumbled on an authority that our French votariſts muſt ſubmiſſively bow to. Mr. Lavoiſier, (in his Eſſays, page 66,) ſays, "Mr. Meyer goes on to point out its *(fire)* combination with a great number of bodies; he ſuppoſes it to exiſt in metallic calces, in minium, and that it may be made to paſs from them either into the fixed or volatile alkalis, which thereby acquire a ſtate of cauſticity. It is principally in this article that Mr. Meyer's theory ſeems to have the advantage over the Engliſh ſyſtem," *(Dr. Black's)*.

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up in their phiſical felicities, the intellectual attainments, and the political conſideration which they conceive to diſtinguiſh their country, that like a great Aſiatic nation, they think that it is beneath them to make themſelves acquainted with the concerns of other countries "——I ſay, What will thoſe men ſay to this,—who have been leagueing with the French Inſtitute to ſuppreſs my diſcoveries, and exalt the French abſurdities; or, in other words, to exalt the French nation and depreſs their own; to ſay not a word concerning the groſs injuſtice of the thing, and Treaſon againſt the philoſophy and the honour of Old England. But I truſt that OLD ENGLAND will ſtill outſtrip FRANCE in SOLID diſquiſition, and yield to her the palm in nothing, unleſs in *aerial* flights, or in ſpeculations that are raiſed upon the moſt trite and frivolous ſubjects.

In all caſes where oxygen gas is formed from metallic calces, either from themſelves or mixed with the acids, either the nitrous, vitriolic or carbonic, fire is required, in order to ſaturate or neutralize theſe acids; and accordingly as you apply the fire, you may expel the acids either by themſelves or with different ſaturations of fire, either as azote or a ſtill purer air, and even with ſo rich an air, or ſo highly impregnated with fixed fire, as oxygen gas.

From the periodical publications I learn that there is a controverſy reſpecting the gaſſes that are abſorbed by water. Mr. Dalton has propoſed a very curious theory, and it is ably ſeconded by Mr. Henry, that “mixed gaſſes neither attract nor repel each other, and every gas is a vacuum to every other gas.”—I think this propoſition an extraordinary one, and too looſely ſtated;—for if ſo, by mixing two gaſſes together of equal bulk, they would only in that caſe occupy the ſpace of one gas. And as to the other propoſition, that “the relation betwixt gaſſes and water is altogether a mechanical one,” I think alſo that it is not juſt. In ſupport of this opinion, I ſhall juſt quote the immortal Scheele*.

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* “Water has the peculiar quality of diſuniting the [next] conſtituent parts of air, in order to unite with empyreal air, and never to enter into union with foul air.

“1. I filled a large bottle with boiled water, which a little while ago had grown cold, and let one-tenth part of it run out; then I ſet this bottle inverted and open in a veſſel with water:—I found the bulk of water diminiſhing ſomething every day; and when it had done diminiſhing, I collected the remaining air firſt in a bladder, and from the bladder into a glaſs; and then introduced a burning candle into the glaſs, which was extinguiſhed the moment it reached the aperture.

“2. Then again I took water whoſe air had been expelled by boiling; filled with it a bottle, and let one-tenth part of it run into a blad-

Now, certainly the gentleman muſt allow a chemical attraction here, and not a mechanical preſſure. Clearly, the water here attracts the empyreal air, in preference to the foul air; and if Mr. Dalton will mix a proportion of fixed air with the atmoſphere, he will find the water have an equal predilection for it, ſuperior even to the empyreal air.

That water has a chemical attraction for airs, I have no doubt. But why they ſhould in *part* eſcape *in vacuo*, I ſhould attribute to the incumbent preſſure of the atmoſphere, acting upon the air chemically united to the water, and condenſing it; but when the preſſure is taken off, the air will increaſe its volume; and therefore the water cannot act ſo forcibly upon it. But it does ſtill on a part of it, I have little doubt. Thus different ſalts are chemically attracted and ſoluted in water; in which caſe if their volume was increaſed by expanſion, I ſhould ſuppoſe the water would precipitate a part of them. This is perfectly conſiſtent with chemical phenomena. That there is a chemical union between water and air, is demonſtrated from fire expelling this air:—it does this, *not* by taking off the preſſure of the atmoſphere, but by che-

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der filled with foul air; then I ſet the bottle inverted into a veſſel with water, and obſerved the ſpace the water occupied in the bottle; a fortnight after I found that the water had not abſorbed any of it.

"3. I put a large bottle (of which the bottom was broke off) into a deep kettle, with water which covered the mouth of the bottle; then I tied a bladder, emptied of air, to the mouth of the bottle, and made the water boil. The air contained in that part of the water which was contained under the bottle, roſe into the bladder; and having tied up the orifice of the bladder, and detached it from the mouth of the bladder, I filled a glaſs with it, and introduced a ſmall burning candle into it;—which burned brighter than in common air." —— SCHEELE's *Chemical Obſervations*, pp. 263, 4, 5.

mically uniting itſelf with the air, (ſtronger than the water does,) and flying off with it. This is done under the preſſure of 30 inches of the mercury.

* Their doing this, is (I ſuppoſe) in part owing to their compreſſing the air into leſs volume: therefore the ſmaller the volume, or the more their ſpecific gravity, the ſtronger the ſaturation.

The explanation of the fact, that a ſtronger ſaturation of fixed air and water can be made by having a column of fixed air over them, rather than the column ſhould be formed of any other air, as the atmoſphere for inſtance, may be accounted for, I think, rationally upon this principle, That *water will ſaturate more of mixed gaſſes than a pure gas*. Therefore, in this caſe, it will attract the empyreal part of the pure air, and let go a part of the fixed air. It is juſt the ſame with ſalts and water: By taking up one ſalt, it will forego part of another. I ſhould ſuppoſe that Mr. Henry will find that atmoſpherical air will make a high ſaturation of water with fixed air, part with more of it than pure azote will †.

Theſe are the firſt ſuggeſtions that have occurred on reading Mr. Dalton's ingenious theory; but I have not conſidered it with all that philoſophic *acumen* I ſhould wiſh to give it, and that it appears to deſerve. My writings have been unjuſtly refuſed admiſſion into the periodical publications; and, as I am now on the eve of publiſhing, I have haſtily thrown theſe hints together, with a view to their being maturely conſidered by the controverſialiſts.

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* Mr. Henry ſays, "It is by no means clear to me, whether or not Mr. Gough denies the principle, 'that the relation between gaſſes and water is a mechanical one.' To me this appears as legitimate an inference as can poſſibly be deduced in phyſics; for the quantity of every gas, abſorbed by water, follows exactly the ratio of the preſſure."—— *Nicholſon's Journal for October*, page 126.

† Mr. Henry ſays, " Certain acid gaſſes,—the muriatic, for inſtance,—are obviouſly excluded."——Pray is not fixed air (or the aerial acid) an acid gas?

Dr. Thompſon's objections to Mr. Dalton's theory, may be all anſwered by Mr. Dalton on the principles of my general theory.

That gaſſes have no attraction for water, is not only a *new* doctrine, but it ſtands in direct contradiction to all former facts and opinions. Thus, *(as I have ſhewn,)* Fire will attract the air that water is ſoluted with, and fly off with it; but that this is done under the preſſure of the atmoſphere, is what no one will deny.—Alſo, take a column of any gas,—the atmoſphere for inſtance,—and expoſe it to a ſtrong heat in a dry bladder; and, after that, it will ſolute again a little moiſture.—*(Proved by an eaſy experiment, and which is well known.)*——Put hot dry air over a column of water, and will it not equally preſs upon the water, hindering its eſcape, the ſame as moiſt air?—With equal propriety might not Mr. Dalton advance, that the preſſure of the air upon the mercury, was by a column or part of the mercury united to the air, acting upon the mercury[*]?

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[*] Mr. Dalton ſays, "The ſubject has never been at all explained that I know of. I have made an attempt which has not yet been publiſhed, except in a lecture laſt winter; but I ſhall ſtill be glad to avail myſelf of any aſſiſtance I can obtain. Suppoſing that in the loweſt ſtratum of an atmoſphere incumbent on water, there is *one* particle of gas for *one hundred* of water, (which may be the caſe, conſidering their relative denſities as *one* to a *thouſand*); *query*, How does the air diffuſe its preſſure over all the hundred particles equally, in ſuch ſort that no column of particles of water is forced up into the interſtices of the atmoſphere by the inequality of the preſſure? If Mr. Gough will explain this, either for

Now Mr. Dalton, I think, has committed an overſight, at leaſt it appears ſo to me—that one pound of feathers could not preſs equally with one hundred pounds of lead. Both a grain of the air and feathers occupy the ſurface of one hundred grains of the water and lead.—Does Mr. Dalton really ſuppoſe that he can, with the point of a needle, the fineſt that can be conceived, touch any point in the incumbent air, where there is *no* air, (or without touching air,) the ſame as the water? Can he not conceive the ſame particle of matter condenſed into a very ſmall volume, and another particle expanded to a large volume? Does not their ſpecific gravity inform him of this ſimple fact? But does he not admit of it, when he ſuppoſes that the little moiſture that the air poſſeſſes, its preſſure upon the ſurface of the ſea, hinders its riſing in the atmoſphere? Does not the water riſe in a ſyphon, when exhauſted of its air? Will not ſpirit of wine do the ſame? And why does not ſpirit of wine riſe up in a tube, even though it contains the air in the atmoſphere. There can be no column of ſpirit of wine in the air. The moiſture in the atmoſphere, he will probably ſay, hinders this. But if different kinds of fluids' preſſure act upon each other, why not the different gaſſes' preſſure act upon each other?

That there is a mutual attraction between the different gaſſes, I make no doubt. The ſame as between the well-known attraction of ſpirit of wine and water; and this for the wiſeſt and beſt purpoſes. That where air becomes mephitic, it will be attracted and diffuſed im-

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his new theory or for any other, I will engage to remove all difficulty on this head which attaches to my hypotheſis."——*Nicholſon's Journal for October*, page 90.

mediately through the neighbouring atmoſphere:—that in Mr. Dalton's example of the two bottles filled with different airs, he acknowledges that it will take ſome time (nay, "hours") before they will be united or incorporated. But does not this imply chemical attraction or diffuſion: for if they were a perfect vacuum to each other, it ought to have been done immediately, as water running to its own level. But it is juſt the ſame as water diſſolving ſugar, or any other chemical operation;—and it is juſt the ſame with the effluvia of bodies in the air: a grain of aſſafœtida will diffuſe itſelf an immenſe diſtance, and a long time, until it is all diſſolved in the air. Is not this a chemical diffuſion, the ſame as a grain of ſalt being chemically diffuſed through immenſe bodies of water*?

Moiſture that is diffuſed in the air, and not held by chemical attraction, muſt be ſuſpended by heat; and it is well known that a colder body will receive this heat from the moiſture condenſing it. Now let any gas be expoſed to a cold body, (as it is by paſſing it through a number of pipes of very cold iron, much below the temperature of the atmoſphere); and after that, expoſe this air to the dry ſalt of tartar, (though its temperature is *above* the air,) or any other ſalt which has a ſtrong chemical attraction for water, and the ſalt will abſorb moiſture from the air. That any cold body condenſes the moiſture in the air, is very diſtinctly ſeen on decanting a bottle of wine brought from a cool cellar: in ſummer the decanter will be covered with moiſture.

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* That dry airs have a chemical attraction for moiſture, I certainly do ſuppoſe. After that ſaturation, the moiſture that is abſorbed by the air, is not chemically united to it; rather, the union is mechanical.—This has been my opinion for years, which probably the world may hereafter be confirmed in.

Well-water is known to contain principally fixed air; and if excluded from the action of light and heat, it will continue to be ſaturated with this fixed air. Though the atmoſphere is not excluded from it, if it was not from chemical attraction, how comes it to retain its fixed air? The air incumbent upon it is the pure atmoſpherical air, which, according to Dr. Prieſtley, Dr. Woodhouſe, and the great and immortal Scheele, poſſeſſes not a thouſandth part of fixed air. Now as this air muſt act as a vacuum over it, how comes it not to part immediately with its fixed air? for the well is always ſurrounded with a column of the atmoſpherical air. Mr. Henry ſays, that water ſaturated with fixed air, does not part with it immediately to the atmoſphere, for this cauſe: As the atmoſpherical air gets united to the fixed air, it acts by preſſure upon the remainder of the water's fixed air.— But let him take a baſon of this ſaturated water with fixed air, and expoſe it in the dark to a conſtant wind, which will inceſſantly ſupply it with freſh atmoſpherical air, and he will find the water ſtill impregnated with fixed air.

The famous well, called, I think, *Grotto del Cani*, will immediately kill a dog; but a man who ſtands erect, having his head higher above the water, is not at all affected. Now in this caſe the mephitic air only extends a certain diſtance over the water, and a dog being lower than a man, breathes this mephitic air, which kills him; but higher, the atmoſpherical air is attracting the mephitic air, ſoluting it; in conſequence, a man may breathe it with impunity. Evidently in this caſe, as the mephitic air is evacuated by the water, it moves the atmoſpherical air, taking its place: but, according to Mr. Dalton, it ought to have paſſed through the atmoſphere's pores without removing it or taking its place. It is juſt the ſame as the mouth of a river flowing into the ſea near its mouth; it removes the ſea water; but at a little further diſtance it is chemically united to it. Or, to reſort to an experiment which is at once eaſy and deciſive: Take a wide-mouthed decanter, which fill with pure fixed air; then take the cork out of the decanter, and expoſe its open mouth to the atmoſphere, and it will be a long time before this fixed air gets ſoluted in the atmoſphere: immediately as it is opened, place a ſmall barometer in it. Now, according to Mr. Dalton's principles, as there is nothing but a column of atmoſpherical air above the fixed air, it cannot act upon the fixed air, as, inſtead of compreſſing it, it becomes a vacuum to it; therefore the barometer ſhould ſhew as if it were in a vacuum: But ſtanding, as it does, at the ſame point as in the atmoſphere, doubtleſs the fixed air ſuſtains the whole weight of the atmoſphere, compreſſing it. Therefore if this fixed air was ſoluted in water, it muſt, *beyond the ſhadow of a doubt*, act equally upon it as it does when it is out of the water. Mr. Dalton ſuppoſes, *(for theories are intoxicating and dangerous things)* that why the water does not riſe immediately into the atmoſphere, is owing to the column of water which the air poſſeſſes; and he ſays, " Mr. Gough next remarks upon my opinion, that " the atmoſphere of aqueous vapour is ſufficient to pre- " vent the ocean from eſcaping into the air, which he " ſeems to think wonderful." Indeed it is ſuch a ſtride of a favourite theory as I have ſeldom witneſſed. In oppoſition to the atmoſphere's compreſſion hindering it, he ſays, The weight or denſity of the atmoſphere is as "*one* to *one hundred*" of the denſity of the water; but *I ſay*, that the quantity or column of water diffuſed in the atmoſphere, *acting upon the water or ſea*, is not equal by any means to the weight of the air; and therefore, upon his own principles, it muſt compreſs the water or ſea only *one thouſandth part*.—If fixed air and atmoſpherical airs were a vacuum to each other, how comes it that the weight of the atmoſphere, which is ſo conſiderable, does not force theſe airs to unite immediately; for in the experiment juſt quoted, (of a decanter being filled with fixed air, and expoſed to the atmoſphere,) it is a long time before they unite: but theſe vaccuums under the immenſe preſſure of the atmoſphere, ought to have made their union immediately; for if a tumbler of fixed air is expoſed to the weight of the atmoſphere, what hinders the atmoſphere from immediately preſſing into it, as their pores are both open to each other, the one being a perfect vacuum to the other.

Mr. Henry found a great difficulty in uniting water and fixed air to that great exceſs. If there was any atmoſpherical air in the fixed air, I ſhould ſuppoſe he would. For, in the firſt place, it is a forced ſaturation by compreſſion and great agitation; therefore the water is over-ſaturated, for which reaſon it is not all chemically attracted: and it is well known in the hiſtory of the ſolution of the different ſalts in water, that one ſalt impedes the ſaturation of the other, exhibiting a very curious variety in the experiments; then why ſhould we be ſurpriſed at the airs doing the ſame? I flatter myſelf that the explanation I have given of the different phenomena accord with the facts, and with all the allowed principles of philoſophy; but that their's is in direct violation of moſt of them.——This is a haſty ſketch. I ſhould have liked to have had more time for reflection, and to have made ſome experiments: but I am forced now to ſubmit my opinions to the world, for reaſons which I have already aſſigned. A patient and candid examination is all I aſk.

Mr. Gough has given a long paper in Nicholſon's Journal, upon atmoſpherical air; its ſuppoſed elements differing ſo widely from the French theory, as to make *this theory* 'appear paradoxical,' as he ſays. True: but if he will examine it by *my* theory, he will find all the facts perfectly accord, and reeceive a rational explanation of all the *unaccountable* phenomena.

I have neither ſeen the publication of Count Rumford, nor that of Mr. Leſlie: the obſervations I am now going to make are deduced from the *Edinburgh Review.* It may be objected, Why not examine the books themſelves? Probably I may do this hereafter; but being on the point of publiſhing, I have little leiſure to procure the books, and ſtill leſs to examine them. . . . . The Count's theories in particular, have ſo much of the *maggot* in them, that what I have ſeen in the Review is ſufficient.

The curious fact that cylinders with poliſhed ſurfaces cooled ſlower than the varniſhed cylinders, or thoſe that were coated with Iriſh linen, I think clearly evinces that *Heat* (according to my theory) is obedient to the ſame laws as *Light;* that the poliſhed ſurfaces retard the admiſſion and emiſſion of both—from the principle of *repulſion*—repelling and hindering both their entrance and exit.

As to the Count's theory of heat and cold being two diſtinct vibrations, and that cold is not a privation of heat,—I ſhall only ſay, in anſwer to ſuch abſurd ſtuff, *Is not nonſenſe a privation of ſenſe?* The Count's philoſophy anſwers, 'NO—they are two diſtinct qualities:'—To which my replication is, That *his head* is coated or varniſhed, ſo as to enſure the emiſſion more of nonſenſe than ſenſe. His notions of æther, and its vibration,—and his elucidation by elaſtic bells,—are other portions of his *ſublime reveries.* Moreover, the Count contends that heat and cold are both vibrations. Common ſenſe then would ſay, that cold is a quality of leſs vibration than heat; and *vice verſa:* that every vibration muſt produce heat, and that cold is a total ceſſation of vibration.

The *elucidation of bells* is of no further uſe, than as it reminds me of a gentleman, an eccentric philoſopher, who wanted to underſtand the language of animals, and to know how they underſtood each other, which he thought was principally by their motions. Conformably with this idea, he put a *vibrating bell* round each of their necks, and alſo round his own: in which way, as the horſe, bull, aſs, gooſe, &c. &c. moved, the bell produced different ſounds, according to their accuſtomed movements. He could with accuracy make *his* bell ring, correſpondent with each particular animal in the concert: ſo that they underſtood each other. And when it happened that the aſs, for example, was paſſing by his ſtudy, it was diverting enough to ſee how ſuddenly he jumped up; and, moving his body ſo as to produce the ſame ſympathetic ſounds, they immediately recognized each other.

That heat is from bodies vibrating, is a poſition which I think I have ſufficiently refuted in my *Thoughts on Air,* publiſhed in 1785—wherein is ſhewn, that a certain degree of heat makes bodies fluid,—and that by extracting this heat, they become ſolid; which muſt completely ſilence all their *Laputatic* ideas, if common ſenſe and reaſon be our guide in our philoſophic reſearches. As to the Count's explanation, it is too puerile to notice ſeriouſly.

May not water, according to the Count's philoſophy, become fluid by the motion of theſe *Lunar maggots;* and when it becomes ice, the maggots have fallen aſleep; for heat muſt be an animated body, that is capable of motion within itſelf: otherwiſe, if heat be inanimate, how could it, when water is turned to ice, receive its impulſe or motion again, to make the ice again become fluid?—That the ſkins of negroes have an oil upon them, from which cauſe they reſiſt the paſſage of heat entering into the ſyſtem, I ſhewed long before the Count, ſo far back as 1781, in my firſt publication.

The Count has given a very extraordinary account and explanation of a drop of water remaining entire longer on very hot iron than on iron heated of a lower temperature. My explantion is this:—In the firſt inſtance, the very great degree of luminous heat of the iron, repels the water, and hinders the iron's attraction for it; for as that luminous heat becomes leſs, the iron attracts the globule, taking from it its globular form,—which then expoſes a larger ſurface to the iron, and in conſequence is ſooner evaporated. But even when it retains its globular form, it is evaporating, yet not ſo quick. Beſides, it does not touch the iron, being repelled from its ſurface. It is preciſely the ſame, if you give a heated ſilver ſpoon a coating of ſmoke with the flame of a candle.—

The ſmoke being of an oily repulſive quality, the water will ſtand upon it in a globular form, not even touching the ſmoked ſurface; the ſame as it would do with oil.— But even in this caſe it is evaporating, for the hot ſpoon is gradually diſperſing it, and this as rapidly as can be expected, in reference to the degree of heat imparted to it. It being globular, and therefore contiguous to the ſmoked ſpoon on one ſmall point of its ſurface,—by placing a cannon ball upon the table, it is ſeen to touch the table only in one *minute* point.

As to the Count's ſingular fact of the ice in the glaciers having a little well of water ſometimes upon its ſurface, it may, I think, be rationally accounted for by particular parts of the mountain emitting heat more than other parts. Have we not hot ſprings in many places? The circumſtance of its being inclined to the plane of the horizon, implies that the heat iſſued from the *ſtrata* of the mountain. Why it was not more than three feet deep, and not above ſeven inches in diameter, is owing probably to the heat of the mountain requiring, *at this part*, THIS CHIMNEY* (as its were) to evacuate its heat; and that the heat was ſo low, and gradual in its paſſage from the mountain, that it could not diſſolve much of the ice, but that therefore it centered at this opening:— for water once formed, (I know from experiment,) has a ſtronger attraction for heat than ice, when below 32 degrees, which it ſometimes is before it freezes, when kept perfectly unmoved. This is one reaſon why ice melts ſo ſlowly: but, to be ſure, the principal reaſon is, *its abſorbing ſuch a quantity of latent heat.*

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[*] "Unlaviſh Nature never works in vain."

But this fact is to be explained by the Count's favourite and important *diſcovery*, that water is *not* a conductor of heat.—Water, and all bodies capable of volatilization, having the greateſt attraction for heat; which is ſometimes ſo ſtrong, that when a particle of water gets hold of a particle of heat, it immediately flies off with it as vapour. But if you hinder that motion, it will attract fire, and conduct it more rapidly than any ſolid body to its neighbouring particles. Thus, if you drop a red hot cannon ball into a large veſſel full of water, and immediately cloſe the mouth of the veſſel ſo as to prevent the water's eſcaping as vapour, it will nevertheleſs receive from the cannon ball its fire far more rapidly than any ſolid body, and the whole of the water will be violently heated by it, which heat it will communicate to all parts of the veſſel, proportionably to its internal heat. Now, how could this be done, if water was not a conductor of heat? To corroborate his doctrine, the Count ſays, that the globules of burning wire, when they fall into water, retain for ſome time their red hot colour. Good Heavens! will any philoſopher ſeriouſly argue, that water cannot readily take the heat from iron, or utterly extinguiſh it?——Why theſe globules of melted iron retain their *colour* for *any* time, is owing to their being ſeveral degrees above 800. Place theſe globules in cold iron, and ſee whether the iron or the water cools them ſooner, and certainly iron conducts heat very readily*. In the higher departments of philoſophy, the Count diſplays neither judgment nor ſolidity, adopting the moſt extravagant flights. The Count *ſhines* moſt when the kitchen fire is at his elbow: indeed, his path in philoſophy ought to be confined to the kitchen; every more elevated province is too ſublime for his genius. But even *in the* KITCHEN I think I can detect in ſome of his improvements too much of *theory* to be uſeful. To him, however, I willingly reſign theſe inferior purſuits.

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* "When an hypotheſis is formed, it is very extraordinary by what vague opinions it is ſupported. In burning iron in oxygen gas, an experiment of Mr. Ingenhouz's, there is an immenſe heat produced, ſo conſiderable as to make the iron run down in red globules; and the iron is ſo ignited, that if it touch the glaſs it will paſs through its ſides as eaſily

In the preſent modern philoſophy the moſt extravagant opinions are adopted, perfectly devoid of common ſenſe and reaſon. Indeed, the more they differ from theſe, the better. Unleſs they have a *Laputatic air*, they are not received.—Dr. Herſchell, when treating of the conſtruction of the heavens, gives very good reaſons to ſuppoſe that they are formed of inſulated ſtars, and double ſtars; alſo of treble, quadruple, quintuple, and multiple ſtars: of cluſtering ſtars, and the milky way: of groups of ſtars, of cluſters of ſtars: of nebulæ, and of ſtars with burs: of milky nebulæ, and of nebulous ſtars: of plane-

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as if it was ſo much wax. (See ſome experiments performed at the Aſkeſian Society.) And Count Rumford ſays, that thoſe globules which flow from the burning wire, if they fall into water, retain for ſome time their red hot colour, bring this as an argument againſt water receiving or conducting fire. Good Heavens! are our modern *aerial* philoſophers to contradict the well-known familiar fact that water extinguiſhes fire?—Why the red globule of iron ſtill retains its colour for a little time, is from its being ſo immenſely heated, that it cannot immediately lower it below 800 degrees, which is the point that iron becomes ignited. And he alſo ſays, there was no hiſſing noiſe from it. Now, noiſe only takes place upon the ſurface of the water; for if he will plunge a red hot poker into water, no noiſe will enſue from it, provided he immerſes it below the ſurface."—*Experiments and Obſervations on Volta's Electrical Pile*, pp. 93, 94.

tary nebulæ, and of planetary nebulæ of centres. But he ſuppoſes all theſe immenſe bodies are held together ſolely by gravitation, and that their motions round each other are regulated alſo by gravitation. Good Heavens! *I aſk this ſimple queſtion*—What hinders their immediately gravitating together into one general maſs? All the laboured calculations of the firſt impulſe being impreſſed by the finger of GOD, aided by a ſuppoſed æther in our planetary ſyſtem, *muſt now* (if there be one grain of ſolid ſenſe amongſt our *aerial* philoſophers) *muſt now vaniſh into "airy nothing;"*—as alſo muſt the abſurd and imaginary flights of La Place, *upon the Newtonian philoſophy*: with all the *French foppery and vanity*, unſupported (as his is) by all geometrical reaſoning,—quite in the ſtile of an *Arabian Tale*:——for, muſt not two bodies, hanging in the heavens, ruſh together, if they are only acted upon by ONE all-powerful, general principle, namely, *Gravitation?* For inſtance; *Will not the* wiſe *heads of our* lunatic *philoſophers* (which are ſo amazingly light) *gravitate to the earth?* Take one of the Tom Fools of Philoſophers, carry him up to the pinnacle of ſome lofty tower; from whence ſuſpend him in the air; and then aſk him if, being left to fall, his philoſophy will preſerve his brains from being daſhed out? Or, what ſupports a philoſopher's head, *even when upon his ſhoulders*, from falling to its mother Earth, if there exiſts not another principle, counteracting gravitation—*the living principle of muſcular motion?* Or, even how could an aſs's head, which is held ſuſpended in the air, having no centre of gravity to reſt upon, be ſuſtained, and preſerved in relationſhip to the body, *if it was not for the vital principle of the animal?* But this is wholly overlooked. All the learned calculations of the ſchools now are ſet at nought, for even many of the heavenly bodies' motions are retrograde, and out of the plain of the planetary movements; as are the newly-diſcovered planets called *aſteroids*, and alſo ſome of the ſatellites of the planets. But that there is a principle of *repulſion* counteracting *gravitation*, is as certain as gravitation itſelf; and that this principle is from FIRE, I have clearly ſhewn. This rationally accounts for the immutable law of ORDER that is eſtabliſhed in the heavens, where *repulſion* keeps the ſyſtem entire, and not a planet can wander from its courſe, nor can one heavenly body interfere with another! = But this *Anglo-Gallic* combination is ſet on foot ſolely to ſuppreſs all my diſcoveries*—which, notwithſtanding their evil machinations, are working their way upon the continent.—— Truth muſt and ſhall prevail†.

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[*] How comes it, *I would aſk*, that there is at preſent ſuch a manifeſt cordiality between the French and Engliſh *wiſe* philoſophers?

……

[†] But why ſhould we be ſurpriſed at the Galvanic fluid taking phlogiſton, or fixed fire, from the metals, as it is an acid fluid. Do not all the acids attract phlogiſton from the metals, viz. the nitrous, vitriolic, muriatic—nay, even fixed air, as the experiments of Mr. Cruickſhanks clearly evince. And I hope I have ſatisfactorily ſhewn, (to every one who is open to conviction) that this nitrous acid may be ſaturated with phlogiſton, in that quantity, ſo as to form what they call the gaſeous oxyde of azote, admitting bodies to burn in it; nay, that it will become inflammable, and explode, as Dr. Prieſtley has clearly ſhewn,—who ſays, "I have mentioned a caſe, (vol i. p. 217.) in which nitrous air, after having been expoſed to iron, became not only partially inflammable, admitting a candle to burn in it with an enlarged flame, but was even fired with an exploſion, like inflammable air from metals by oil of vitriol. I have ſince met with a more remarkable fact of the kind"—— So he goes on to relate it.

Alſo it appears from Dr. Prieſtley's experiments, quoted pp. 274, 5, that the gaſeous oxyde of azote is much altered, both by agitation in water, and alſo by the electrical fire, becoming in conſequence more reſpirable, and alſo acted upon by nitrous air. Now the airs that are formed by the Galvanic pile, muſt be operated on by both agitation and the electrical fire.

It is very extraordinary how our *aerial* chemiſts ſtill adhere to the French theory. Mr. Cheveneux ſays, in his experiments on platina, that even water deoxygenates it. So water has a ſtronger attraction for oxygen than the calx of a metal! I ſee that they aſſert from their experiments, that the calx of platina poſſeſſes the calx of two other metals. I ſhould ſuppoſe that it is a deception that one part of the platina calx has got a high ſaturation of the fixed alkali, and the other of the volatile alkali: in conſequence, they form calces of peculiar qualities, that is, different from the common calx of platina, and that they are both real calces of platina.

Mr. Nicholſon is dragged in to obſerve—"Profeſſor Tromſdorff has noticed that metals are combuſtible by means of the Galvanic ſpark in hydrogen, ammonia, nitrogen, nitrous and carbonic acid gaſſes."——*Journal for September* 1803, page 62.——Now this is what I ſhewed long ago, that metals are calcined in theſe airs. Here then is combuſtion and calcination, without oxygen. This is cutting up their favourite theory by the roots. But our votariſts are ſo infatuated as to believe it cannot err. The falling of ſtones is a circumſtance which has intereſted our *aerial* philoſophers;—Mr. Izarn ſuppoſes them generated by the exploſion of airs. This is no other than ſuppoſing, what common ſenſe revolts at, that, in the atmoſpherical elaboratory, water is decompounded and compounded, alſo ſtones are formed!

That the acids are taken up by the electric fluid, and circulate with it through the different metals of the pile, (as alſo does the calx of metals,) I have clearly proved in my Galvanic Experiments. The French philoſophers found the cancerous matter of ulcers alſo circulate with it through the metallic plates; and Lieut. Col. Haldane ſays, "The green oxyde of copper in the tubes was examined by putting ſome drops of ammoniac *(aqua ammoniæ puræ)* into each of the tubes; the green oxyde was diſſolved, and the water acquired a blue tinge, but ſome flakes of a brown ſubſtance remained in the tubes, not acted upon by the ammoniac. This brown matter was not always formed by all combinations, but the ſilver and copper always produced it." —— It appears from the continental publications, that ſome foreign philoſophers have diſcovered the ſame.

My treatment has been ſeverer than any philoſopher (I believe) ever experienced; and when I come to give a full hiſtory of it to the public, they will then be ſenſible how much I have ſuffered. This has provoked, at times, very pointed and ſevere animadverſions; but I mean to be perſonal to no one; I only ſpeak *to* men, and *of* men, collectively, as a body.

My work has been very near ten months in the preſs; — a diſadvantage to which publications undertaken in the country are frequently ſubject: I have endeavoured, however, to get it out to ſtart at the ſame time with the Lecturers, in the hope of defeating the views of unfair traders and plagiariſts. It is rather a ſuſpicious circumſtance that ſo many gentlemen ſhould make *a diſcovery* much about the ſame time, and we wonder how they got it! As, for example, the *ſuppoſed* diſcovery of the compoſition of water—that Wonder tranſcending all the Wonders we have named!

As Cervantes had the honour of ſuppreſſing the *Quixotiſms* of Chivalry, ſo I aſpire after no leſs an honour than that of ſuppreſſing the *Quixotiſms* of Science, which are equally baneful and extravagant.

I ſhould think myſelf under the greateſt obligation to any ingenuous and candid reader, who would inform me how I can get this book tranſlated,+ and diſperſed over the continent: I am particularly deſirous that it ſhould fall into the hands of Mr. Ritter, who is certainly a very acute philoſopher. Dr. Crell, I hope you will make it known by means of your diſtinguiſhed Journal. If there be any candour, or honour, or juſtice, or the love of truth in ſcience, I ſhall ſurely call them forth.

That water has a chemical attraction for air, is evinced by Scheele's experiments, that the empyreal air was abſorbed by the boiled water; and that it was not from the preſſure of the atmoſphere, is certain—for no foul air was attracted or abſorbed under the ſame circumſtances.

I am glad to find reſpectable men becoming Editors of Reviews. I ſee Mr. Arthur Aikin is the Editor of the Annual Review. I certainly may expect candour from a man of his character and ſituation in life. There is a groſſneſs in the common Reviews which every diſcerning perſon muſt reprobate, and a partiality that is truly diſguſting.

+poor Man! how much to be pitied!

They uſed regularly to notice Crell's Chemical Journal, as well as the French Chemical Annals. It is obſervable, however, that ſome years have elapſed ſince they noticed the former work,—becauſe it has of late breathed ſentiments hoſtile to the French theory, diſplaying a ſpirit above being the *tool* of *any* ſyſtem or party.——And now I am diſtributing to every one his due,—Mr. Tilloch, you uſed to give your readers the different papers which were read at the Royal Society: Why have you deſerted this practice? I believe the reaſon is, They refuſed to publiſh a valuable paper of Dr. Crell's, which corroborated ſome experiments of mine, and which behaviour was remarked by me.

The mind of the philoſopher, I have often thought, being more extenſive than is that of the vulgar and uninformed, of conſequence it ought to be more exalted, and ſhould be the reſidence of every generous quality. If, in the midſt of all his attainments, real or imaginary, he is deſtitute of honour, candour, integrity, juſtice and truth, he is deſpicable indeed!

Any man who mentions the French theory after this, without naming my publication, I aſſert, poſſeſſes neither juſtice, candour or honour,—and is at the ſame time an enemy to ſcience. Madneſs ind

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ADDENDA.—I have juſt ſeen the Monthly Review for September, wherein it notices, in part, Mr. Leſlie's treatiſe on Heat—but it is to be continued. I have there learnt ſome of the doctrines of Mr. Leſlie.—His experiments are ſomething ſimilar, I ſuppoſe, to thoſe of Count Rumford: the *priority* they themſelves can ſettle.——Mr. Leſlie found, that his heated caniſters, with hot water, when received by his reflector, gave out heat differently, according to the different ſurfaces of the caniſter, the ſame as the Count's cylinders. Now this I have accounted for by ſuppoſing that heat is repelled the ſame as light, only with different impulſes: that poliſhed metals hinder its emiſſion moſt from the caniſters, juſt the ſame as it would do light: that when covered with paint, it being a ſolid body, it therefore would receive the heat from the caniſter better than the air; conſequently, the ſide of the caniſter would become hotter, and impel more heat upon the reflector. This is further confirmed from the circumſtance of the ball of his differential thermometer equally receiving the heat,—in the like degree as the caniſters emit it, according to the ſame coverings:——which confirms my opinion.

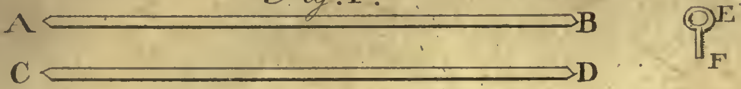
According to Mr. Leſlie's theory, heat is conducted by an elaſtic body, as air imparting the heat of the caniſter to the reflector, by impulſes or undulations; and ſo from the reflector to the thermometer.—He ſets aſide the Count's abſurd and imaginary Æther, and at the ſame time gives ſome ſtriking experiments to ſhew its fallacy. Adopting the undulations, by ſuch means the heat is carried by the elaſtic fluid air.—Now, if we are not totally to renounce all former experience, the reflector muſt receive and impart the heat in ſtraight lines, and not by undulations. If it were by undulations, (as Sound,) we ſhould have an inſtrument in theſe experiments, whereby the impulſes or undulations might be conveyed, the ſame as we convey ſounds—ſomething anſwerable to a trumpet. But here we are aſſiſted with only the ſame inſtrument which we uſe to convey light, namely, a reflector. In theſe very diſcoveries, I think, Mr. Leſlie, your head wanted *a reflector*, or, at leaſt, *you* wanted *reflection*. That heat is reflected the ſame as light, has been abundantly proved by Dr. Herſchell's experiments: for how otherwiſe could the heat in theſe experiments be thrown upon the thermometer? By furrowing the ſurface of the caniſter, you would only in this caſe form a large ſurface of caniſter to emit the heat to the reflector. That heat is the ſame as light, only the latter has more impulſe, I think I have demonſtrated in my former works; and that heat will not paſs through water like light, I have clearly ſhewn is from its impulſe being leſs; of conſequence, the chemical attraction for water arreſts it, and from its great maſs or body, it forcibly attracts it; but air, being ſo much lighter, it requires leſs to heat it. Why the caniſter did not act upon the thermometer, when cooled by a current of air, is owing to this—The conſtant current of air took from it its heat, therefore it could not impart it to the caniſter: for to be impelled from the ſurface of the caniſter, the caniſter muſt poſſeſs a high degree of heat to repel it. As iron, when heated above 800 degrees, gives out its heat as light; but when below that, its heat is emitted *as heat*.——But, to every candid mind, I have clearly proved that *heat* is *light*, with a leſs impulſe—ſo *vice verſa*. And Mr. Leſlie, like Dr. Young, probably wanted to adopt my theory, with modifications: juſt as the Doctor did with my Theory of Colours, by the introduction of Æther.——Thus, from a haſty peruſal of the Review, I have ſtruck out this imperfect ſketch. Opportunity will not allow me to expand this ſubject to a great length; indeed I have ſaid as much as it deſerves.

[F. Jollie, pr. Carliſle.

FINIS.

Plate I.

Fig. I.



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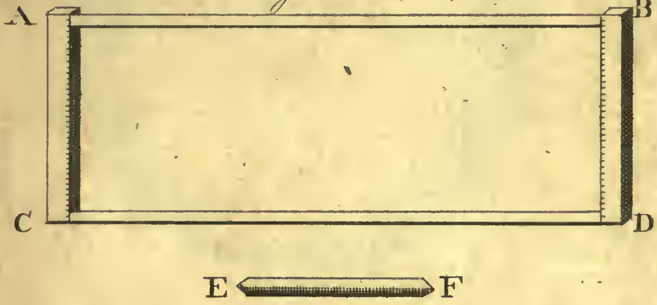

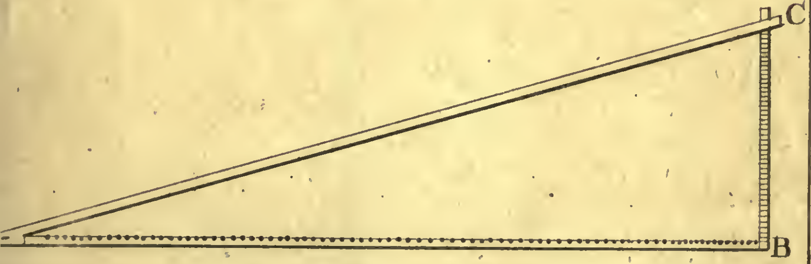

Plate II

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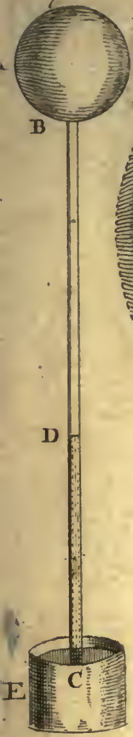

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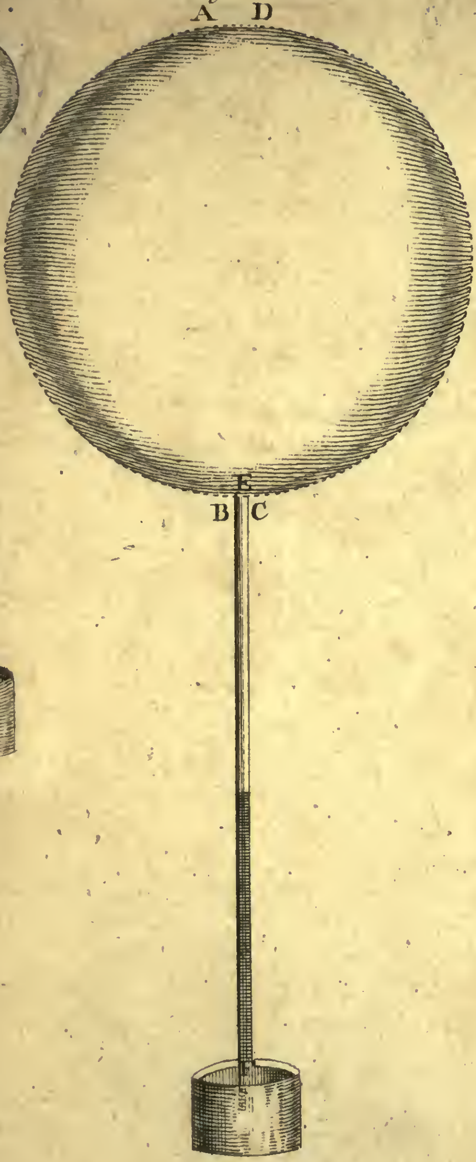

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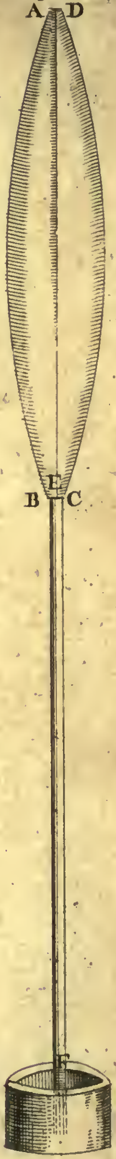

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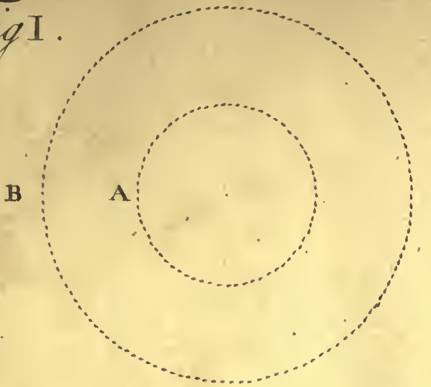

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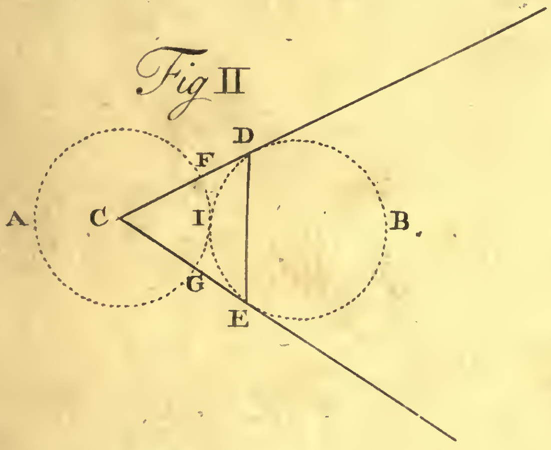

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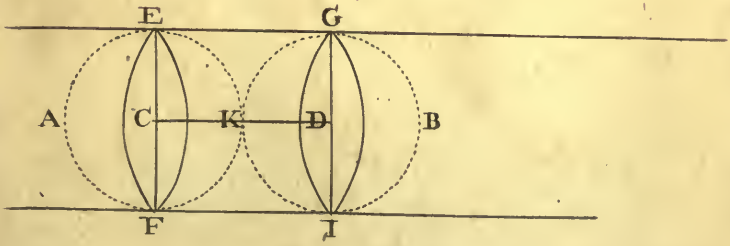

Plate. IIII

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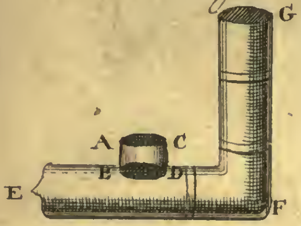

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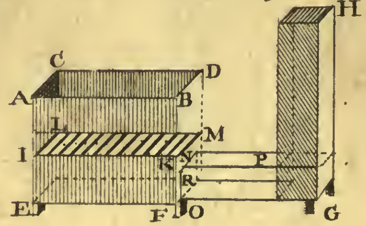

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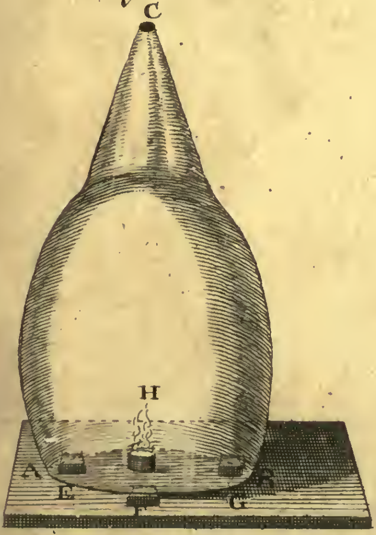

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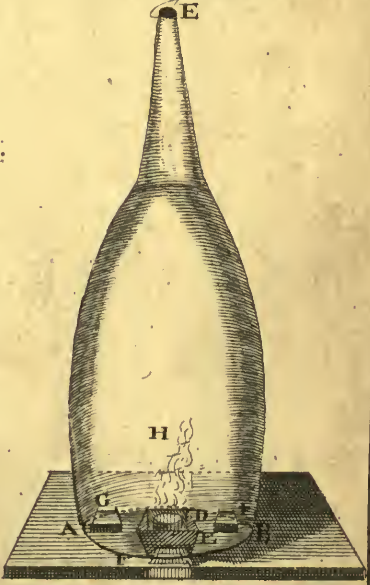

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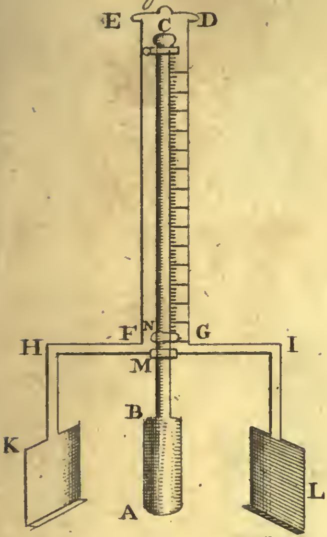

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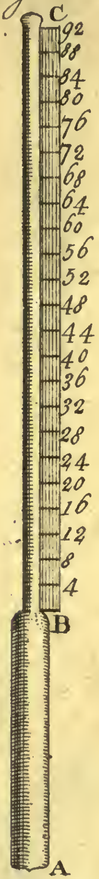

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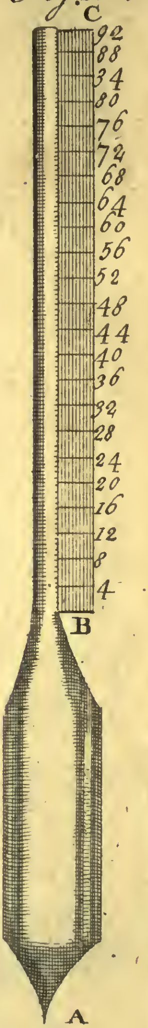

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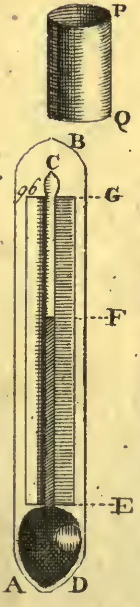

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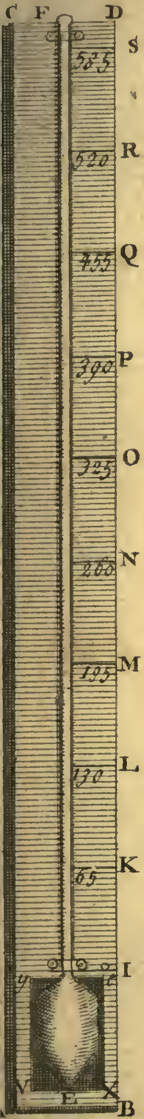

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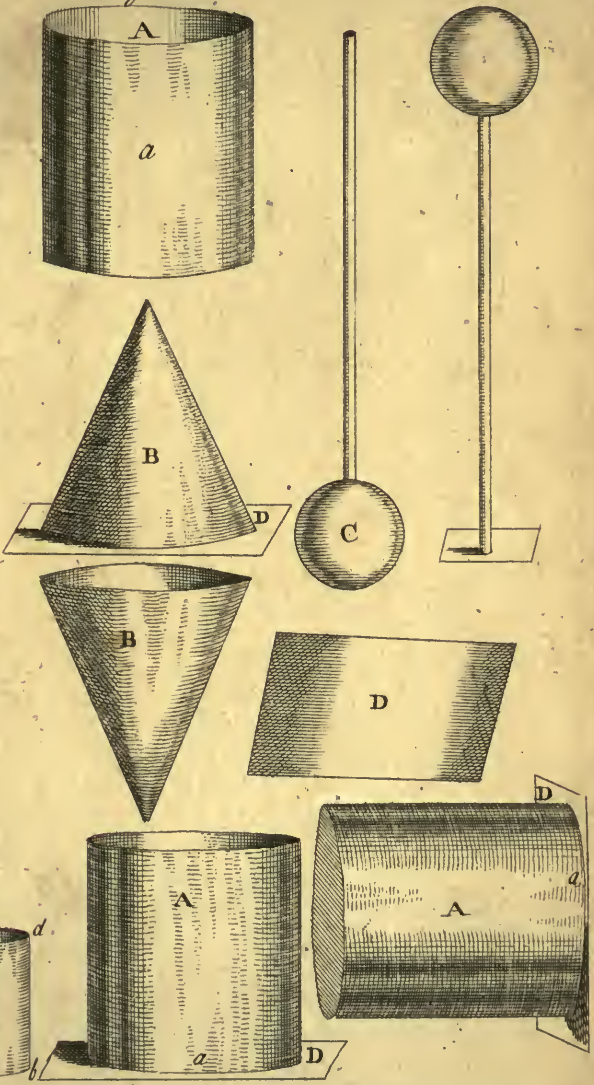

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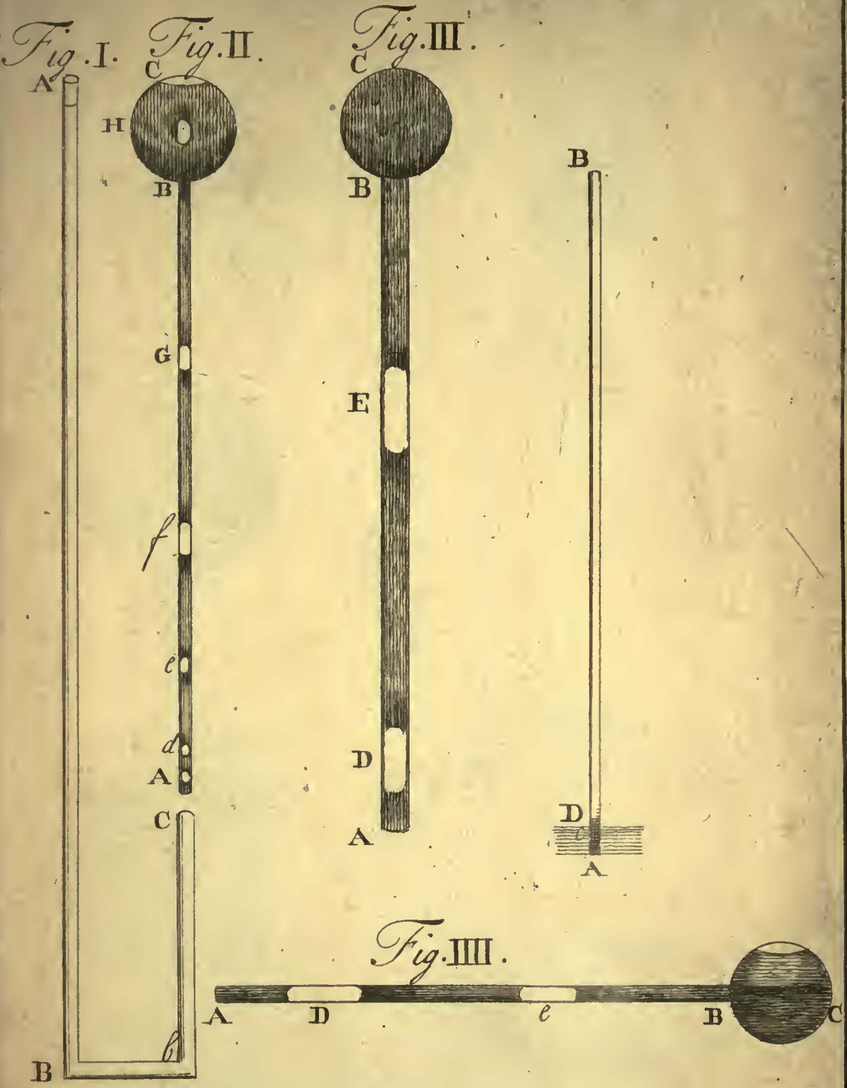

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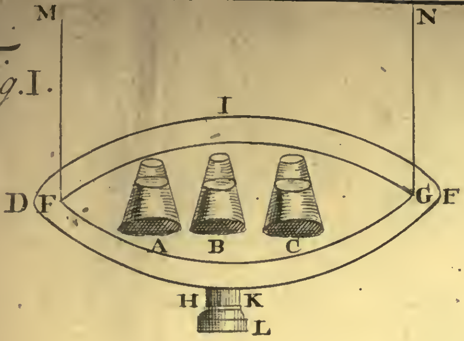

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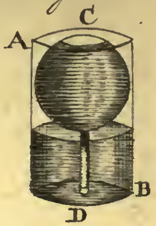

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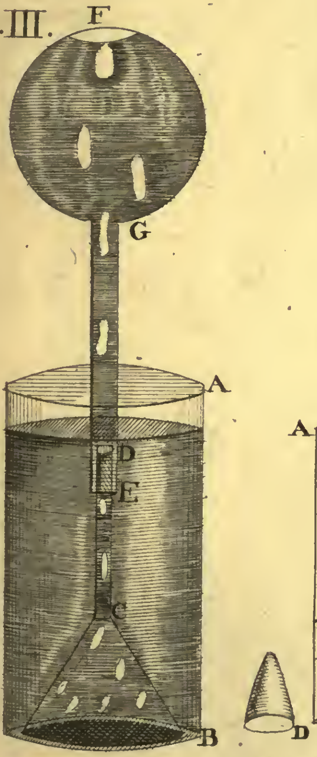

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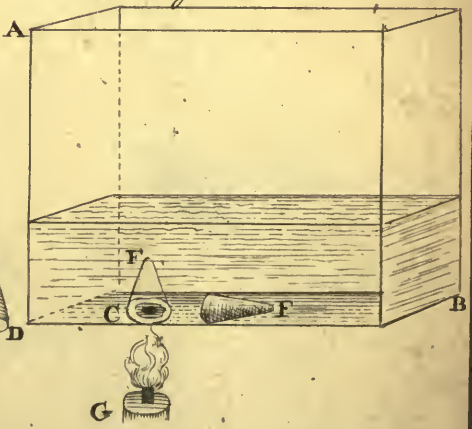

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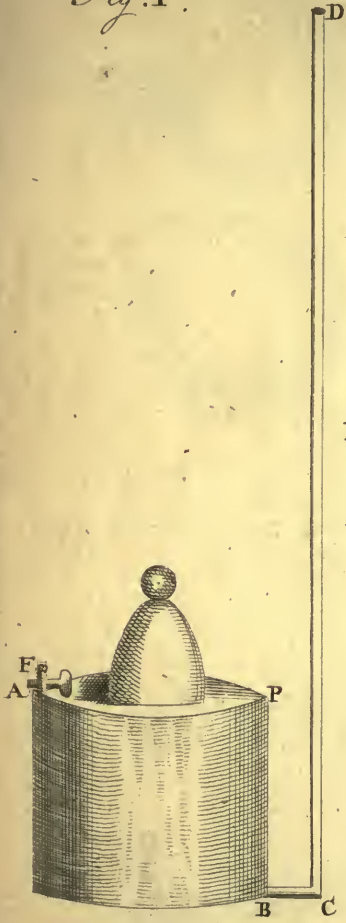

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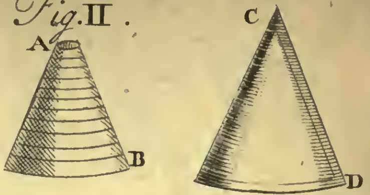

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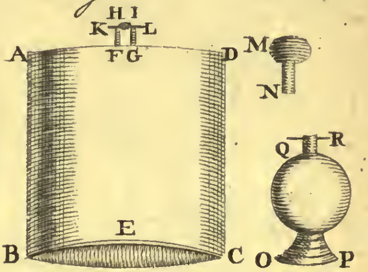

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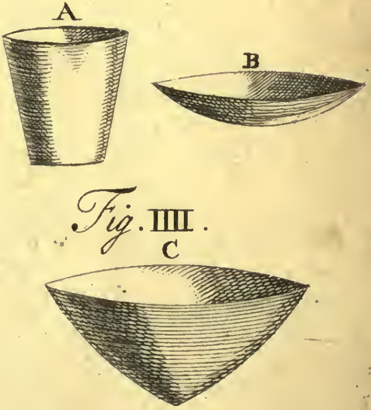
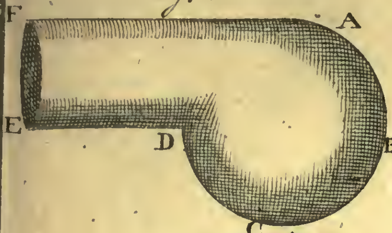
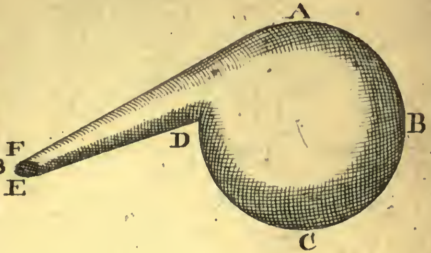


Plate X.

Fig. I.





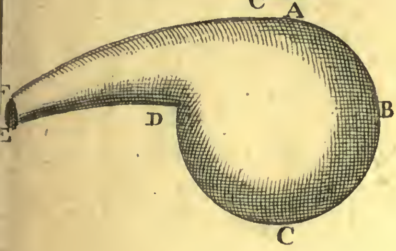
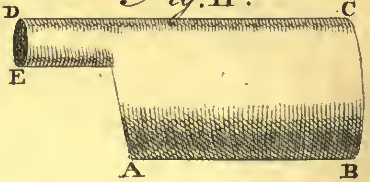
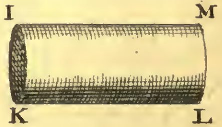


Fig. II.





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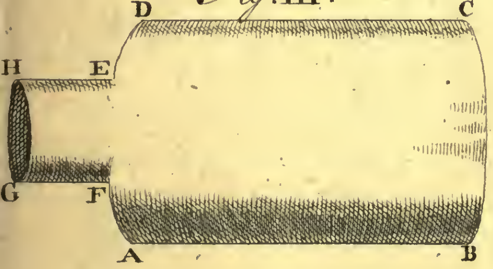


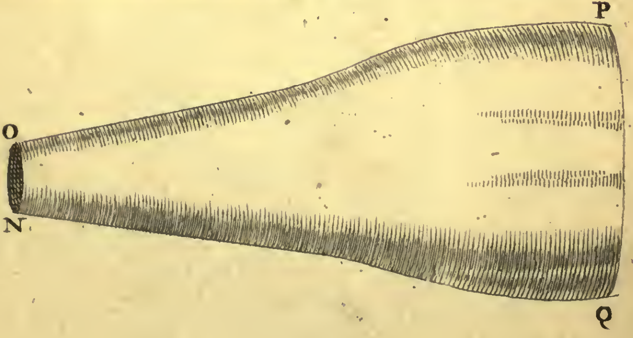
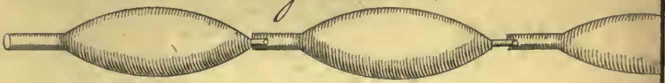


Plate. XI.

Fig. II.



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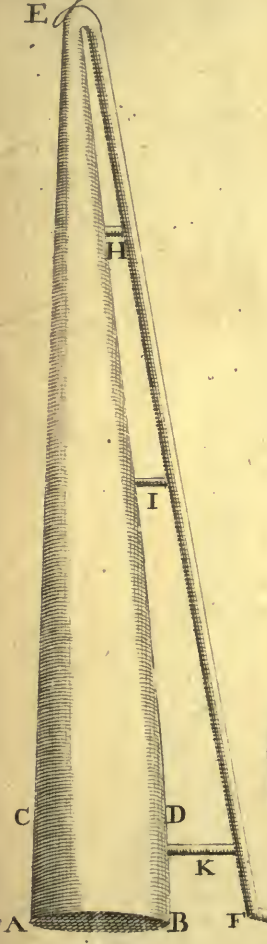

Fig. III.

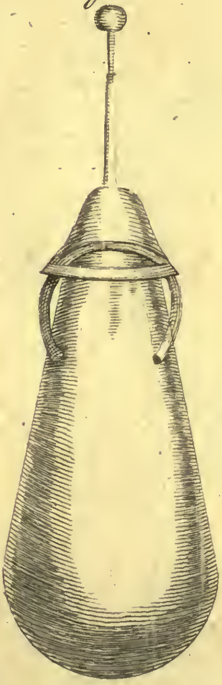

Fig. IIII.

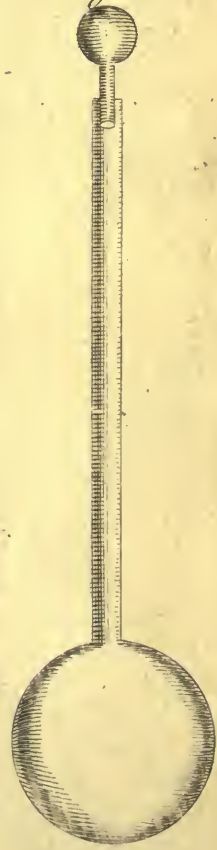

Fig. II.

Fig. I.

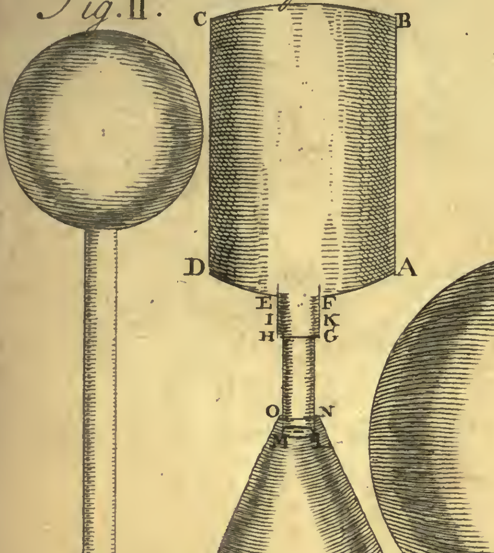

Fig. III.

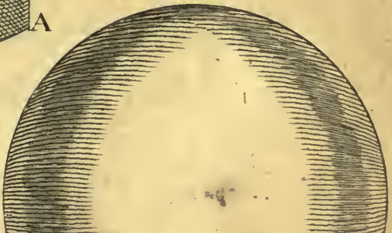

Plate XIII.

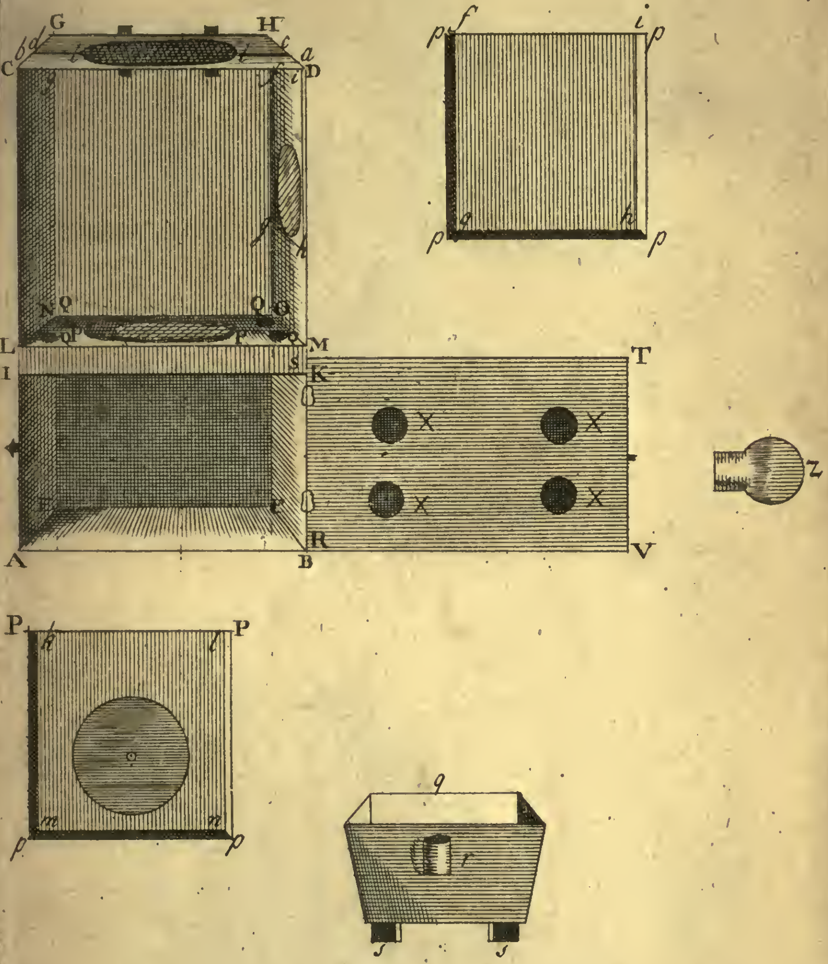
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Plate XIIII

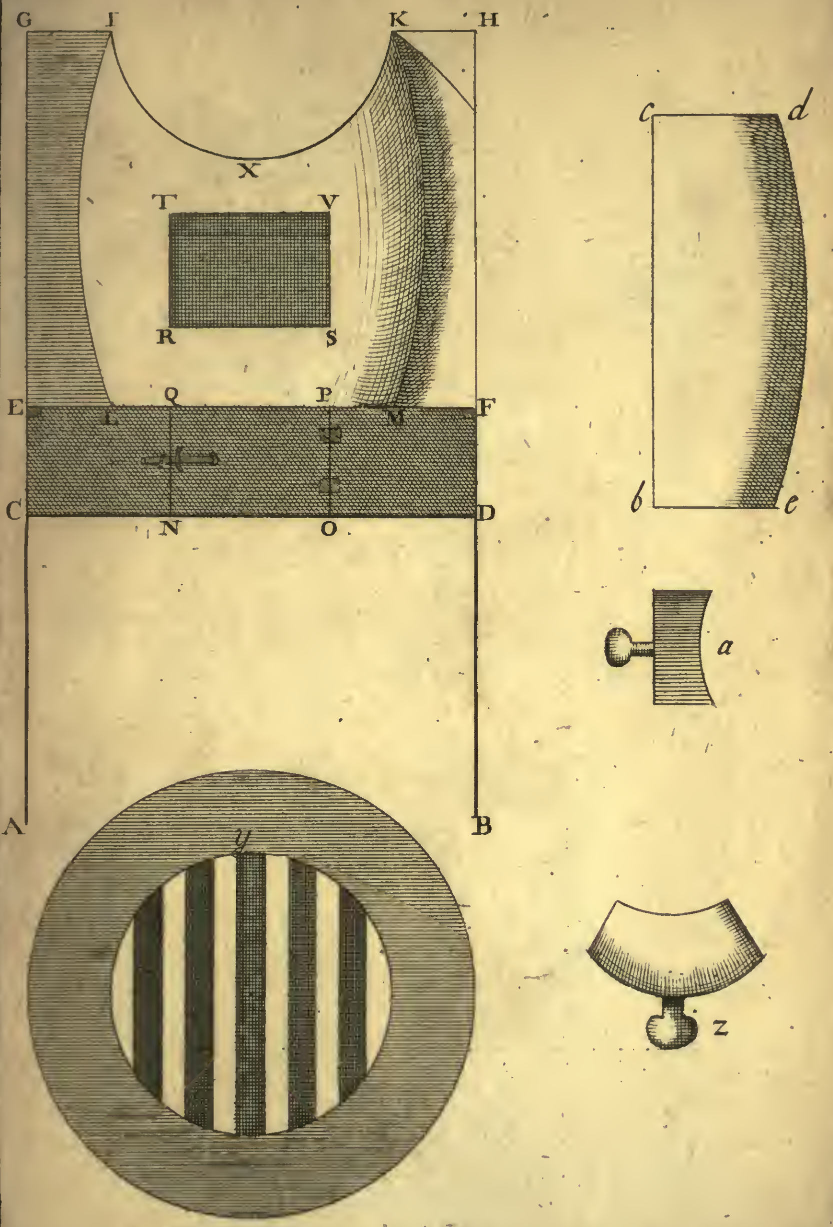

Plate XV.

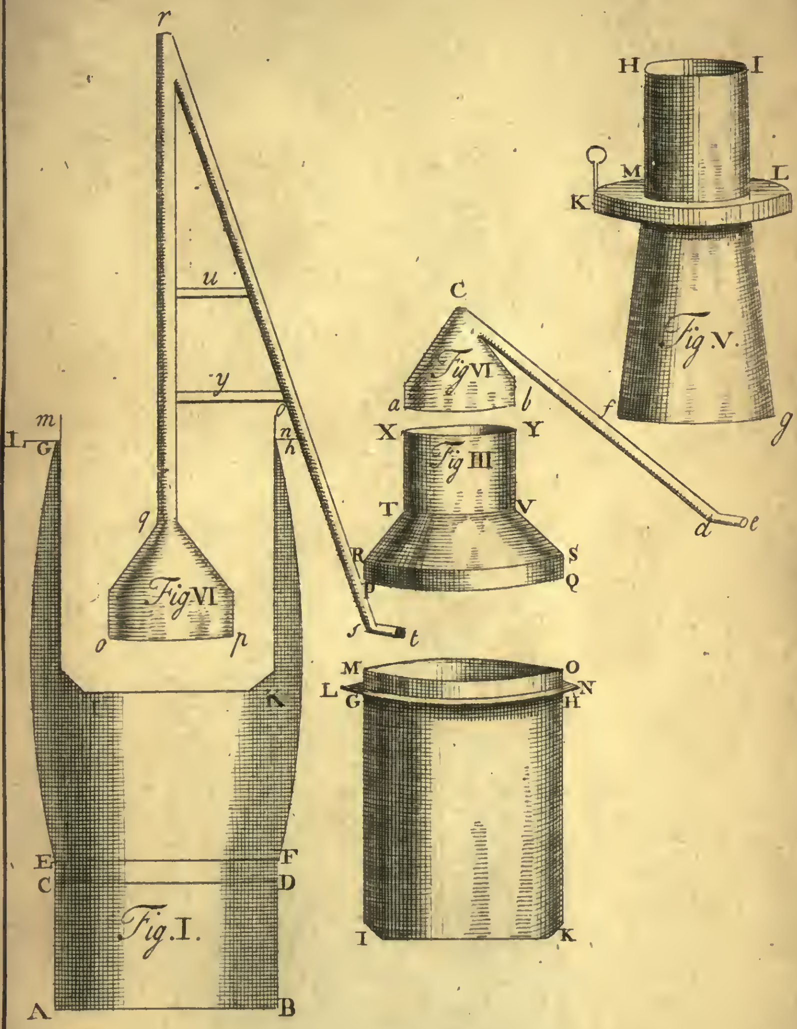
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Plate XVI

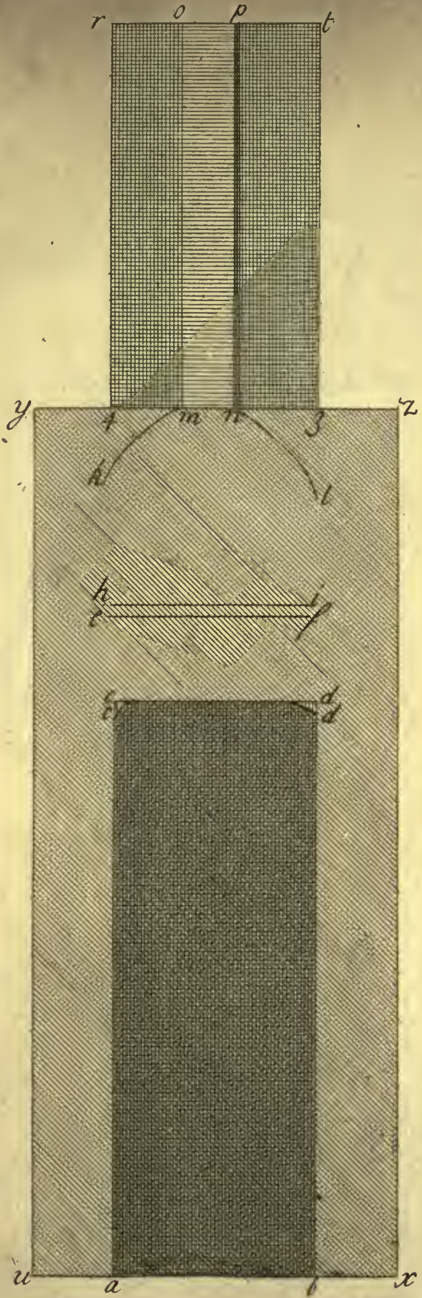

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Plate XVII

Fig. I.

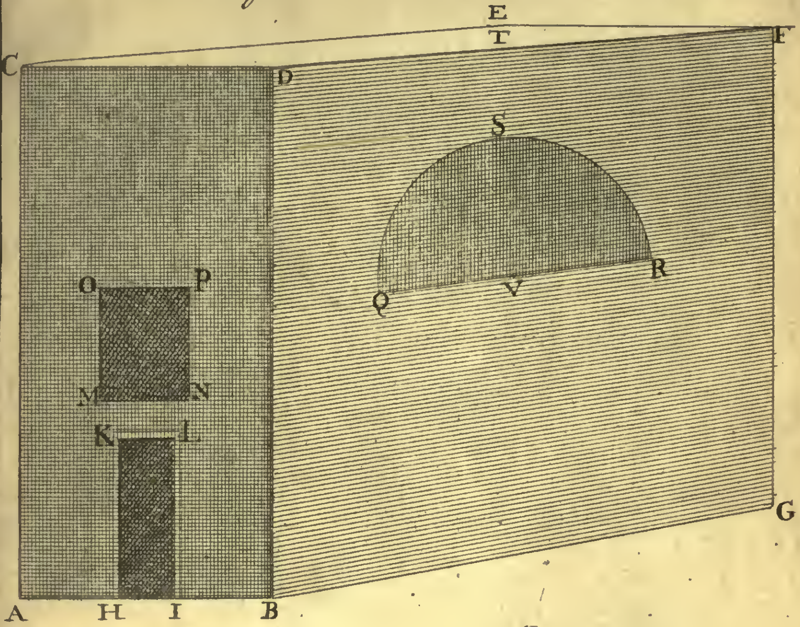
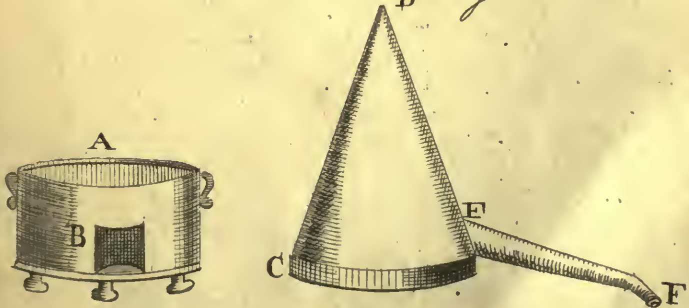


Fig. II.



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